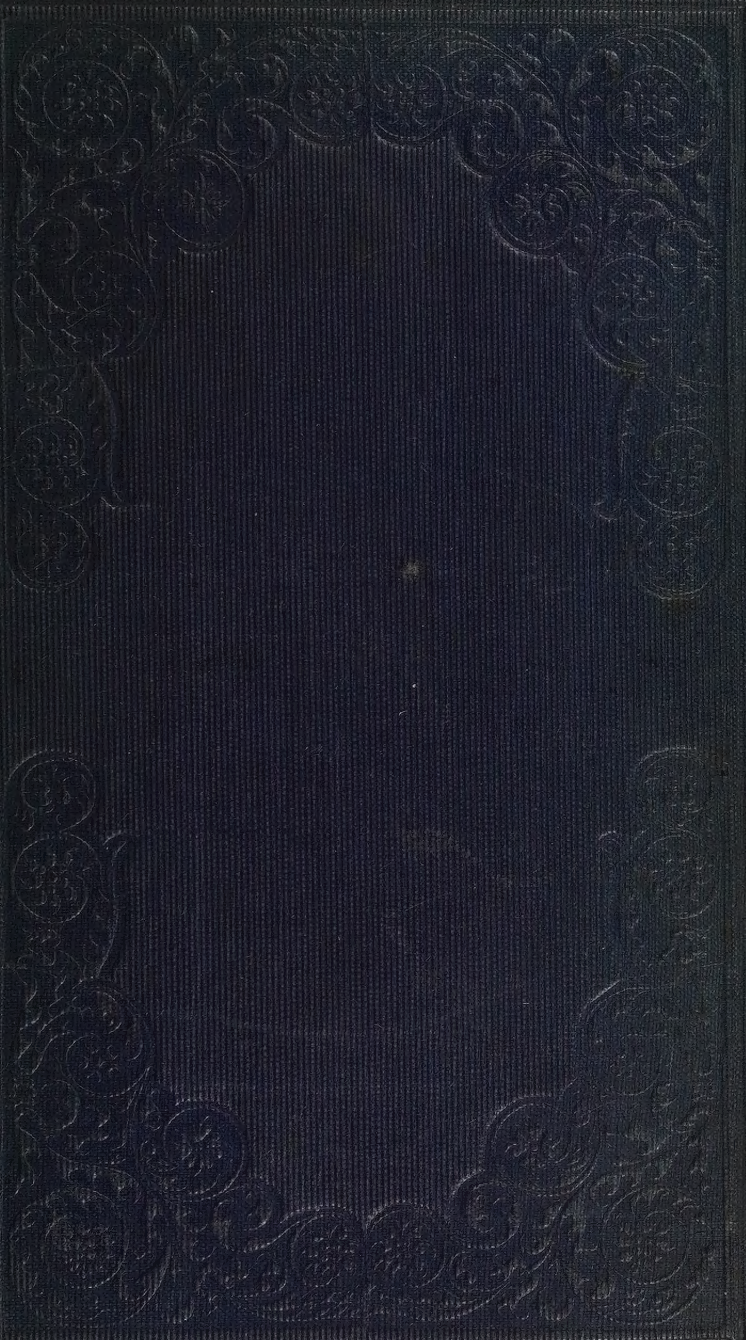

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BRAITHWAITE'S RETROSPECT.

VOL. LIII, JANUARY—JUNE, 1866.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. LOND.

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CONTENTS OF VOL. LIII.

SYNOPSIS.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ARTICLE.		AUTHOR.	PAGE.
1	On Fevers	*Dr. H. Bence Jones*	1
2	On the Thermometer in Specific Fever	.. ..	7
3	Thermometrical Observations on the Fevers of Children	*G. Stevenson Smith, Esq.*	11
4	On the Use of the Thermometer as a Guide in the Diagnosis of Pyrexial Diseases	*Dr. F. W. Gibson*	20
5	Observations on the Present Epidemic of Typhus ..	*Dr. Robert Perry*	27
6	Researches on the Daily Excretion of Urea in Typhus Fever, with Remarks	*Dr. Keith Anderson*	22
7	On Typhus Fever	*H. W. Randolph, Esq.*	35
8	On an Outbreak of Typhoid Fever with Special Reference to its Etiology, and Complication of Diphtheria	*Dr. D. Campbell Black*	37
9	On the Treatment of Typhoid Fever	*Dr. T. King Chambers*	40
10	On Acute Rheumatism and Acupressure	*Donald Macgregor, Esq.*	42
11	On Blistering in Acute Rheumatism	*E. Howard Moore, Esq.*	43
12	Citric Acid as an Application in Cancer .. .	*Charles J. Denny, Esq.*	44
13	On a Case of Removal of Cancerous Tumour of the Arm	*John Hilton, Esq.*	45
14	Polydipsia Treatment by Large Doses of Valerian—Improvement	*Dr. Reith*	47
15	On the Treatment of Disease by Oxygen	*Dr. R. H. Goolden*	49

DISEASES OF THE NERVOUS SYSTEM.

ARTICLE.	AUTHOR.	PAGE.
16 On Progressive Locomotor Ataxy, or Wasting of the Posterior Columns of the Spinal Cord	*Dr Julius Althaus*	51
17 A Case of Progressive Locomotor Ataxy	*Dr. Johnson*	58
18 A Case of Progressive Locomotor Ataxy	*Dr. Hughlings Jackson*	59
19 On Spasm	*Dr. C. Handfield Jones*	61
20 On the Causes, Diagnosis, and Treatment of Functional Nervous Affections	*Dr. C. E. Brown-Sequard*	67
21 On the Moral Treatment of Functional Nervous Diseases	*Do.*	72
22 On the Use of Bromide of Ammonium in the Sleeplessness of Functional Nervous Diseases	*Do.*	73
23 Cases Illustrating the Use of Belladonna and Bromide of Potassium in Epilepsy	*Dr. Broadbent*	73
24 On the Treatment of Epilepsy	*Dr. C. E. Brown-Sequard*	75
25 On the Use of Chloroform in Epilepsy and Hysteria	*Do.*	79
26 On the Use of Ice Bags in Convulsions and in Cholera	*Dr. John Chapman*	80
27 Chorea Treated by Calabar Bean	*Dr. John W. Ogle*	83
28 Subcutaneous Injection of Narcotics for Neuralgia	*Do.*	85

ORGANS OF RESPIRATION.

ARTICLE.	AUTHOR.	PAGE.
29 On the Nature, Cause, and Treatment of Tuberculosis	*Dr. Horace Dobell*	86
30 New Bi-Valve Speculum for Examining the Larynx, Invented and Patented by Dr. Labordette, of Lisieux	.. ..	100
31 On the Thermometer as a Means of Diagnosis between Tubercular and Non-Tubercular Diseases	*Editor of Medical Times and Gazette*	100
32 On a Certain Form of Hemoptysis Unassociated with Pulmonary Tuberculosis	*Dr. R. Payne Cotton*	105
33 On the Use of Phenic (Carbolic) Acid for the Cure of Phthisis	*Dr. J. R. Wolfe*	108

ORGANS OF DIGESTION,

ARTICLE.	AUTHOR.	PAGE.
34 Concerning the Treatment of Constipation and of Stoppage of the Bowels—with Special Reference to the Use of Atropia and of Galvanism	*Dr. Alex. Fleming*	109
35 On the Treatment of Diphtheria with Hyposulphite of Soda	*J. C. Maynard, Esq.*	119
36 On Tracheotomy in Croup and Diphtheria	*Dr. George Buchanan*	120
37 On Hepatic Disease amongst Europeans in the East Indies	*Inspector-General Sir Ranald Martin, C.B.*	125
38 On Wheat Phosphates	*Dr. Tilbury Fox*	130

URINARY ORGANS.

ARTICLE.	AUTHOR.	PAGE.
39 On the Symptoms arising from Alkaline Urine	*Dr. H. Bence Jones*	131
40 On the Treatment of Ammoniacal Urine	*Do.*	132
41 On the Symptoms and Consequences of Interstitial Nephritis	*Do.*	134
42 On the Treatment of Tubular Nephritis	*Do.*	137
43 On Secondary Albuminuria (Cause not Primarily in the Kidney)	*Dr. George Harley*	138
44 On the Treatment of Bright's Disease	*Do.*	144

SURGERY.

DISEASES OF BONES AND JOINTS, &c.

45 On the Use of Chloride of Zinc in Surgical Operations and Injuries	*C. De Morgan, Esq.*	147
46 Forcible Flexion of the Knee for Anchylosis in the Straight Position	*Dr. Philip C. Smyly*	162
47 Subacute Synovitis and Contracted Joints	*W. Adams, Esq.*	163
48 On an Easy Method of Reducing Ordinary Dislocations of the Shoulder Joint	*W. E. Porter, Esq.*	164
49 On the Treatment of Fracture of the Radius at the Styloid Process by Means of Gordon's Splint	*Lawson Tait, Esq.*	165
50 On some Injuries to the Head and Neck of the Humerus	*J. Hutchinson, Esq.*	168
51 On a New Form of Leg-Suspender and Bed Guard	*Henry Greenway, Esq.*	172
52 Leather-Felt Splints		175
53 How to Make Gutta-Percha Splints	*Edwin Truman, Esq.*	176
54 On a Case of Recurring Fibroid Disease of the Shoulder	*Sir William Fergusson*	177

ORGANS OF CIRCULATION.

55 On the Galvano-Puncture of Aneurism	*Dr. John Duncan*	180
56 On Ligature of the Femoral Artery	*Prof. Syme*	189
57 On the Treatment of Popliteal Aneurism	*John Hilton, Esq.*	191
58 On a Case of Wounded Ulnar Artery, followed by Sloughing and Secondary Hemorrhage, and requiring Amputation	*Do.*	194

ORGANS OF URINE AND GENERATION.

ARTICLE.	AUTHOR.	PAGE.
59 On the Proofs that Lithotrity is an Eminently Successful Operation	*Henry Thompson, Esq.*	195
60 On a New Instrument for Facilitating the Operation of Lithotomy	*Henry Lee, Esq.*	197
61 On the Treatment of Acute Orchitis by Puncture and Compression (Immediately after Puncture) by Strapping	*Dr. W. Spencer Watson*	198

DISEASES OF THE SKIN.

62 The Actual Cautery for Nævi	*J. R. A. Douglas, Esq.*	203
63 On the Nature and Treatment of Chilblains . ..	*Dr. Henry S. Purdon*	201
64 Cases of Pruritus Senilis	*Dr. H. Beigel*	202
65 On Eczema	*John Gorham, Esq.*	203
66 Treatment of Acute Eczema	*T. P. Johnston, Esq.*	203

DISEASES OF THE EYE AND EAR.

67 On Examining the Fundus Oculi	*Dr. Roseburgh*	204
68 On the Treatment of Granular Ophthalmia by Pressure	*Dr. W. Stokes, Jun.*	204
69 On the Treatment of Ophthalmia	*Ernest Hart, Esq.*	208
70 On Black Cataract	*Haynes Walton, Esq.*	209
71 On the Treatment of Strabismus without Operation	*C. Holthouse, Esq.*	210
72 On Atropine Points	*J. Z. Laurence, Esq.*	212
73 Blue Glasses	.. ..	212
74 Treatment of Malignant and Recurrent Tumours of the Orbit	.. ..	213
75 A New Otoscope or Speculum Auris	*Dr. John Brunton*	214
76 On a New Method of Applying Remedial Agents to the Cavity of the Tympanum	*Dr. Edward Bishop*	216
77 On Difficulty of Hearing in Cases of Perforation of the Membrana Tympani	*Dr. F. E. Weber*	217

SYPHILITIC DISEASES.

78 On Syphilitic Inoculation in 1865..	*Henry Lee, Esq.*	221
79 On the Treatment of Syphilis	*Do.*	228
80 On Syphilization	*James Lane, Esq.*	237

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

ARTICLE.		AUTHOR.	PAGE.
81	On Puerperal Fever	*Dr. Snow Beck*	241
82	On the Use of Wire-Ribbon in Some Cases of Difficult Turning	*Dr. Val. Heyerdahl*	246
83	A Remarkable Case of "Missed Labour"; the Fœtus being Retained in the Abdomen Forty-Three Years	*R. W. Watkins, Esq.*	249
84	On Enlargements of the Uterus which follow Abortions, and Premature or Natural Confinements; with Cases	*Dr. Snow Beck*	251
85	On Hemorrhage from Detached Placenta near the Eighth Month of Gestation: Labour Induced by Barnes's Dilator: Recovery	*W. J. Tubbs, Esq.*	253
86	Abstract of a Paper on Procidentia Uteri	*Dr. J. Marion Sims*	255
87	On the Radical Cure of Prolapsus Uteri	*Robert Ellis, Esq.*	260
88	On the Operation for the Radical Cure of Prolapsus Uteri by Caustic Applications	*Dr. C. H. F. Routh*	264
89	Case of Polypus Uteri: with Remarks	*Dr. J. G. Swayne*	270
90	On a New Uterine Support	*Dr. William Main*	275
91	On Extreme Flexions of the Uterus	*Dr. Henry G. Wright*	279
92	A New Uterine Dilator	*J. W. Hembrough, Esq.*	286
93	On Vaginal Rectocele	*James R. Lane, Esq.*	287
94	A Case of Ovariotomy in which the Pedicle was Tied and Returned, and the Ligature Removed in Forty-Eight Hours	*Dr. J. H. Aveling*	294
95	On Chlorate of Potass in the Treatment of Ovarian Disease	*W. Craig, Esq.*	296
96	On the Treatment of Ovarian Tumours	*I. Baker Brown, Esq.*	300
97	On Iodine in the Treatment of Uterine Leucorrhœa	*Dr. Murray*	300
98	On Dysmenorrhœa, Metrorrhagia, Ovaritis, and Sterility, depending upon a Peculiar Formation of the Cervix Uteri; and the Treatment by Dilatation or Division	*Dr. Robert Barnes*	301
99	On Improvement of the Blood in Chlorosis, and on the Elimination of Certain Poisons	*Dr. C. E. Brown-Sequard*	314
100	Glandular Tumour of Mamma, with Cysts; Injection with Compound Tincture of Iodine	*W. Martin Coates, Esq.*	315
101	Cure of a Large Ranula by Injection of Compound Tincture of Iodine	*Do.*	316
102	On a Case of Spina Bifida cured by Injection of a Solution of Iodide of Potassium	*Do.*	317
103	Practical Remarks on the Loosening of the Articulations of the Pelvis	.. ..	320
104	On the Mortality of Childbed as Affected by the Number of the Labour	*Dr. J. M. Duncan*	323
105	On Liebig's Food for Infants and Invalids .	*Dr. A. Hill Hassall*	326

ADDENDA.

ARTICLE.		AUTHOR.	PAGE.
106	On the Citric, Acetic, and Carbolic Acids in Cancer	*Dr. John Barclay*	329
107	On the Uses of the Medicinal Sulphites and Hyposulphites in Zymotic Diseases	*Dr. Polli*	332
108	Dr. Hassall's Flour of Meat		338
109	On the Concentration and Preservation of Meat	*Dr. A. Hill Hassall*	339
110	On the Preservation of Milk	*Dr. J. Scoffern*	341
111	A Case of Resuscitation, after Drowning, by a Combination of the Plans of Dr. Silvester and Dr. Marshall Hall		342
112	On the Action of Fungi in the Production of Disease	*Dr. Tilbury Fox*	343
113	Obstruction of the Bowels, Successfully Treated by the Use of the Stomach Pump	*Dr. Ebenezer Fleming*	357
114	On the Sphygmograph	*Editor of the Lancet*	359
115	Therapeutical Notes	*Dr. Headland*	360
116	On the Physiological Operation of the Preparations of Iron	*Dr A. Sasse*	361
117	Sinapine Tissue, or Mustard Paper	*— Cooper, Esq.*	363
118	On the Subcutaneous Injection of Morphia and Atropine	*Dr. C. E. Brown-Sequard*	363
119	On the Absorbing Power for Medicines possessed by Certain Parts of the Body	*Do.*	363
120	Chloroform as a Means of Detecting Bile in the Urine	*M. Cunisset*	365
121	An Antidote for Prussic Acid		365
132	On Alcohol	*Dr. Henry Munroe*	365
123	The Course of a Dose of Alcohol through the System	*Dr. H. Bence Jones*	368
124	On a New and Ready Mode of Producing Local Anæsthesia	*Dr. B. W. Richardson*	369
125	On Rhigolene, a Petroleum Naphtha for producing Anæsthesia by Freezing	*Dr. Henry J. Bigelow*	384
126	On Anæsthesia by Mixed Vapours	*Robert Ellis, Esq.*	386

INDEX.

GENERAL INDEX, VOLUMES XLVII—LII.

SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—The germs of the disease lie scattered far beyond the apparent limits of the tumour, and hence, the apparent return of the disease is but the continued development of germs which were not included in the extirpation of the tumour. This is not merely theory, for on examining the tissues in the neighbourhood of such a tumour, little deposits of the morbid structure may frequently be seen, quite distinct from one another, and varying in size from the smallest pin's head to a large grain of wheat. In one instance some of these deposits were at least 3½ inches from the original seat of the mass. Probably the reason why an operation appears to intensify the disease, is, that during it germs are set free and are carried by the lymphatics or veins, or in the areolar tissue, and becoming arrested here and there, take root, and are developed into new tumours. If this view, that cancer is a local disease, which becomes disseminated from the point of its first invasion is correct, the practice of early extirpation cannot be too much insisted on. After removal of such tumours by the knife, a solution of chloride of zinc, twenty to forty grains to the ounce, should be applied to the wound and thoroughly pressed in with a sponge; the surface will soon feel soft and creamy, and any germs of the disease will be destroyed. Chloride of zinc produces very little irritation beyond the point to which it is applied; it may therefore be worked into the inner surface of a flap till the tissue is softened to within a few lines of the surface. (Mr. C. De Morgan and Mr. Moore, pp. 147, 150.)

Citric Acid.—In cases of ulcerating and painful cancer, as of the mouth, breast, or uterus, a solution of citric acid locally applied will give complete relief. (Mr. C. J. Denny, p 44.)

Citric, Acetic, and Carbolic Acids.—Citric, acetic, and carbolic acids, when applied in a diluted state to cancerous tumours, have a powerful effect in removing pain. The carbolic acid has a powerful effect in correcting the offensive fœtor of cancerous discharges. All three have a solvent effect on cancerous tissue, the citric acid least, the acetic acid next in degree, and the carbolic acid most. When a weak solution of carbolic acid is applied to cancer cells under the microscope, they are dissolved and the nucleus even disappears almost entirely. In a case of mammary tumour, to which it was applied, the thick, serrated, and everted edges disappeared, and cicatrization of many of the sores occurred. The following is the best formula:—Acidi carbolici ʒiss.; spiritûs vini rectificati ℥j.; aquæ ad ℔. ij. (Dr. J. Barclay, p. 329.)

Convalescence from Inflammation.—Nitric acid is the most suitable medicine known for the state of convalescence from inflammation, and in the various cachectic states following on acute disease, or on habits of intemperance. Its beneficial influences may be referred to its possessing a large proportion of oxygen in loose combination, which promotes a more free circulation through diseased parts, or through effused solids, by further oxygenating them. (Dr. C. J. B. Williams, p. 128.)

Fever in Children.—*Diagnosis of, by Thermometer.*—The average temperature of the body in children from three to ten years of age is 99°. In cases of real fever it rises rapidly to 102°, 103°, or 104°, whereas it remains at or below the normal temperature in cases where real fever is not present, although there is foul tongue, quick pulse, and other symptoms of continued fever. Here we have a ready and accurate means of diagnosis. (Dr. G. S. Smith, p. 11.)

Gout.—*Inhalation of Oxygen.*—The formation of urea instead of uric acid may be determined by supplying oxygen. Oxygen may be directly inhaled, or the binoxide of hydrogen exhibited in one-drachm doses diluted in about two ounces of water. This has a marked influence on the biliary secretion, which it increases in quantity and improves in quality, often producing excessive biliary dejections, thus relieving congested livers and secondary bronchial congestion. The inhalation of a mixture of oxygen and air, in the proportion of one to four, pretty uniformly clears the urine of lithates. (Dr. R. H. Goolden, p. 49.)

Typhoid Fever.—The most successful plan of treatment of typhoid fever (only one case in forty proving fatal) is the following: Strong beef tea and milk every two hours, together fourteen pints every twenty-four hours, and twenty drops of dilute nitro-muriatic acid every two hours. The late Dr.

Henderson, of Shanghai, stated that by its adoption the mortality from continued fever was diminished from 28 per cent. to 7 per cent. (Dr. T. K. Chambers, p. 40.)

Temperature in Typhoid Fever.—Typhoid fever is not present, if, on the evening of the first or second day of the disease, the the temperature be 104°; if the evening temperature do not rise to 103·3° between the fourth and sixth days; if, in the second half of the first week, the evening temperature decrease considerably; lastly, if, between the eighth and eleventh days, the temperature be below 103°. (Dr. F. W. Gibson, p. 20.)

TYPHUS FEVER.—*Amount of Urea Excreted.*—During the second week of typhus fever the quantity of urea excreted daily is decidedly below the standard of health, notwithstanding the presence of a state of high fever. This is contrary to the generally received doctrine that fever consists essentially in an increased metamorphosis of tissue. The intensity of the head symptoms coincides with the lowest urea excretion, and their improvement coincides with or follows on an increase in in that secretion. (Dr. K. Anderson, p. 32.)

Temperature in Typhus and Typhoid Fevers.—The temperature of the body affords more reliable information respecting the severity of a case of typhoid fever than either the tongue or the pulse. It is also of great use in diagnosis. Thus, if on the fourth or fifth day the maximum temperature attained during the twenty-four hours be not 103·5°, the disease is most probably not typhoid fever, whilst, on the other hand, if on the first or second day the maximum temperature reaches 104° the disease is some other than typhoid fever. During the second week the thermometer is of great use as a means of prognosis. The greater the fluctuation between the morning temperature (lower) and the evening temperature (higher), the more favourable is the attack, and the shorter will be its duration. In cases of persistent elevation of the temperature, the earliest sign of improvement is a fall of the morning temperature. (Dr. Ringer, p. 8.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—*Calabar Bean.*—The calabar bean promises to be an effectual remedy for chorea. It should be given in half-drachm doses of the tincture (ʒj. of the bean to ℥i. spirit of wine) three times a day, in water, and gradually increased if necessary. In one case which had gained no ground for more than two months this plan of treatment produced most marked improvement in a fortnight, and cure in four weeks.

In some cases no benefit seems to follow its use, but this may be said of any medicine. (Dr. J. W. Ogle, p. 83.)

Epilepsy.—*Turpentine.*—Turpentine is a very valuable remedy in epilepsy. It may be given in half-drachm doses three times a day, or in a single dose of three drachms at occasional intervals, combined with an equal quantity of castor oil. (Dr. Headland, p. 361.)

Epilepsy, Hysteria, Neuralgia.—In cases with a distinct aura, a narrow circular blister applied all round a limb, a toe, or a finger, or a circular cauterization with a white-hot iron may cure the case, or at any rate be productive of benefit. (Dr. Brown-Séquard, p. 76.)

Functional Nervous Disorders.—*Mental Occupation in.*—In cases of hypochondria, of hysteria, of chorea, and even of epilepsy, a great benefit can be derived by inducing the patient to have some serious aim, whereby the mental faculties may find healthy employment. (Dr. C. E. Brown-Séquard, p. 72.)

Bromide of Ammonium.—It is of the greatest importance to procure sleep in patients attacked with a morbid increase of the reflex excitability. In cases in which there is sleeplessness owing to some cause of cerebral excitement, sleep is almost invariably induced by giving to adults a dose of thirty grains of bromide of ammonium a quarter of an hour before the last meal, and a second dose of from thirty to fifty grains at bedtime. (Dr. C. E. Brown-Séquard, p. 73.)

Lumbago.—*Ether Spray.*—If there is no gouty or rheumatic condition, instant relief is sometimes attainable by the use of Richardson's ether spray. (Mr. J. B. Walker, *Lancet*, March 17, 1866, p. 299.)

Neuralgia.—The application of the ether spray of Richardson's apparatus for producing local anæsthesia gives immediate relief in cases of neuralgia. (Dr. B. W. Richardson, p. 376.)

Subcutaneous Injection of Atropine.—In cases of neuralgia not only may morphia be injected subcutaneously but aconitine in doses of $\frac{1}{20}$ of a grain, and atropia in doses of $\frac{1}{30}$ of a grain, may be similarly employed, and sometimes with benefit, where morphia has failed. (Dr. J. W. Ogle, p. 85.)

Pain.—*Subcutaneous Injection of Morphia and Atropine.*—Inject together, half a grain of the sulphate of morphia with one-sixtieth of a grain of sulphate of atropine, and the good effects of both against pain are obtained, without the bad effects. (Dr. C. E. Brown-Séquard, p. 363.)

Progressive Locomotor Ataxy, *or Wasting of the Posterior Columns of the Spinal Cord.*—In a well-marked case of tabes dorsalis (so called by Hippocrates) there is an actual waste of nerve fibres of the posterior columns of the spinal cord. The principal symptoms are a want of co-ordination of voluntary movements, and a tendency on the part of the patient to lose his balance, but without much actual loss of power. The only remedy which seems to be of any avail is the nitrate of silver, given in doses of one-tenth to half a grain two or three times a day; it may be continued for four or six weeks consecutively, and then discontinued for a fortnight or three weeks. The gums must be examined from time to time, as the peculiar coloration which silver produces in the long run first appears in the mucous membrane, and only afterwards in the skin. It acts better when combined with the hypophosphite of soda. The disease was supposed by Hippocrates to arise from excesses in sexual intercourse. It, however, also arises from exposure to cold, exhaustion, and rheumatism. (Dr. J. Althaus, p. 51.)

Until recently this disease has been included under the class paraplegia. The symptoms however do not consist in actual paralysis of the muscles, but loss of power to guide them. If the patient shuts his eyes he stops and sometimes falls. The microscope shows disease of the posterior columns of the spinal cord. There is no loss of sexual power. (Dr. Johnson, p. 59.)

Spine.—*Effects of Cold and Heat to the.*—Cold and heat applied to the spine produce definite and constant effects, exactly the opposite of one another. Ice applied to the spine increases the general circulation, stops the cramp of voluntary and involuntary muscles, proves an effective remedy in epilepsy and other convulsive affections, cures sea sickness, restrains the sickness of pregnancy, arrests diarrhœa, recovers patients from the cold stage of cholera, and finally, promotes menstruation. On the other hand heat along the spine lessens the general circulation, overcomes congestion in all parts of the body, lessens fever, restrains hemorrhage, and lessens or arrests the menstrual flow. (Dr. J. Chapman, p. 80.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Aneurism.—In tying the femoral artery the greatest nicety is required, and this will ensure almost perfect safety. First expose the sheath of the vessels carefully, next close every little vessel that discharges enough to obscure the parts; then seize the sheath with dissecting forceps, and gently rasing it, make a small opening by means of a straight narrow sharp-pointed knife. Elevate the cellular and fatty substances which envelope the vessels until the external coat of the artery appears quite distinct and *white*, when the needle may be passed with ease and safety. (Prof. Syme, p. 189.)

Galvano-puncture.—In cases of aneurism, especially those which cannot be reached by surgical means, as compression, the only successful plan of treatment open is that by galvano-puncture. Introduce from the same side two needles into the sac, parallel to each other, and from one to two inches apart. The needles should be carefully insulated. They should be extremely fine, but may be multiplied according to the size of the aneurism. They should be made of steel, coated with gold, as the steel is eaten away with great rapidity, and the surface acting is thus diminished. A battery of medium strength, and with a continuous current, should be connected with the needles. The action may be continued till pulsation stops, or gas can be clearly detected by percussion. In a case of aortic aneurism related, the current was passed two hours and ten minutes. (Dr. J. Duncan, p. 180.)

Popliteal Aneurism.—Let the leg be bandaged and elevated, the patient confined to bed, and a grain of opium be given at bed-time if restless. Bend the knee at an acute angle so as entirely to stop pulsation in the aneurism. A pad may also be strapped over the tumour, and if necessary, a double tourniquet applied over the superficial femoral artery. By simple flexion at an acute angle, all pulsation through the tumour is arrested, and this the more perfectly the higher the tumouris situated in the popliteal space. A tumour situated in the middle of the space, that is, at that spot which is opposite the interval between the tibia and femur, is not curable by this plan of treatment. (Mr. J. Hilton, p. 191.)

CONGESTION.—*Internal Use of Chloroform.*—Chloroform given by the stomach and rectum does not produce any symptoms of anæsthesia, but has the effect of equalizing the circulation of the blood and so relieving congestion. It may be thus used in all cases of congestion whether sthenic or asthenic. (Dr. Merril, *Lancet*, *Nov.* 25, 1865, *p.* 605.)

HEMORRHAGE.—*Xylo-Styptic Ether Spray.*—A solution consisting of absolute ether, having a boiling point of 92° Fahr., charged to saturation at a low temperature with tannin, and, afterwards treated with xyloidine, a little short of saturation, produces good local anæsthesia, and has the effect of producing complete coagulation of the blood in the surface of the wound exposed to the spray. The primary effects are of course those produced by the cold, namely, the condensation and whitening of the tissues. If blood be flowing it solidifies, and when the parts relax, new blood that may ooze up enters the solid blood as though it were a sponge, quickly solidifying by coagulation and stopping further flow. The effect is so powerful that even when the spray is directed on partially defibrinated and decomposing blood, solidification ensues. Dr. B. W. Richardson, p. 382.)

Hemorrhage from Wounds of the Hand.—In cases of hemorrhage from the hand, occurring from several points simultaneously, flex the forearm forcibly upon the arm, and the hemorrhage will cease, owing to the acute angle at which the radial and ulnar arteries are placed. (Mr. J. Hilton, p. 193.)

PYÆMIA AND SEPTICÆMIA.—In pyæmia and septicæmia, and all the class of fevers with putridity and purulent absorption, such as hospital fever, puerperal fever, anatomical inoculation, and the like, give sulphite of magnesia so as to saturate the organism with it rapidly, and cause the fluids of the body—urine, saliva, sweat—to contain it. It will rapidly check the effects of putrefactive absorption. (Dr. G. Polli, p. 334.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

CHRONIC BRONCHITIS OF THE AGED.—When, in chronic bronchitis in old people there is great dyspnœa, accompanied by a dry state of tubes, the tincture of stramonium given in ten minim doses along with other expectorants is very useful. (Dr. Headland, p. 361.)

CROUP AND DIPHTHERIA.—*Tracheotomy.*—Whatever may be the primary affection, whether sthenic croup, diphtheritic croup, or genuine diphtheria, death occurs in fatal cases from one of two causes, viz., exhaustion or suffocation. At an early period of those cases, the type of which tends to death by suffocation, before the energies of the patient have been wasted by the progress of the disease, perform tracheotomy. If you wait longer it will be too late. (Dr. G. Buchanan, p. 120.)

DIPHTHERIA.—*Hyposulphites.*—The incipient exudation of diphtheria may frequently be removed in forty-eight hours by the application twice a day of a strong solution of hyposulphite of soda, viz., ʒiij. of the hyposulphite, glycerine ʒij., with ℥vj. of water. A weak gargle may be used every hour. (Mr. J. C. Maynard, p. 119.)

LARYNX SPECULUM.—An instrument has been invented by Dr. Labordette, of Lisieux, in appearance like a vaginal speculum, but adapted to the throat, and having a mirror so placed that when introduced into the mouth the larynx is reflected. A great deal of light is economised by reflection from the polished interior of the speculum tube. It may be obtained of Maw and Son, of Aldersgate-street. (p. 100.)

TUBERCULOSIS.—Tuberculosis is owing to imperfect or abnormal pancreatic secretion, whereby the fatty part of the food is imperfectly prepared for absorption. This is frequently aided by an unhealthy condition of the intestinal mucous membrane. When the case is so far advanced that it is considered hopeless to aim at restoring the function of the pancreas and the absorbing power of the intestine, we must give pancreatic emulsions of solid fat, or of oil, in the hope that some absorbing power for such matter may yet remain in the digestive tract. The proportion of oxygen in the air presented to the lungs must be diminished in order to preserve the albumenoid tissues from disintegration to the greatest possible extent, and for the same reason artificial warmth must be supplied. If the symptoms, however, are only premonitory, the principles of treatment must be quite the opposite of those detailed: the function of the pancreas must be restored as quickly as possible by placing the patient under those conditions which call for pancreatic action, these are an atmosphere rich in oxygen, a climate at once cold and dry, exercise, cheerfulness, and a good mixed diet. The bulk of cases, however, are intermediate between these two, and require a nice adjustment of the plans of treatment. (Dr. H. Dobell, p. 86.)

Diagnosis of Tubercular Diseases by the Thermometer.—The temperature is always preturnaturally raised when tubercle is being deposited in any of the organs of the body; this elevation often continues for many months. The temperature reached in tuberculosis varies from any point above that of health to 105° or 106° Fahr. The usual maximum temperature of the day varies between 102° and 103° Fahr. An appeal to the thermometer will not unfrequently decide the diagnosis in a case of suspected tubercular deposit in the meninges of the brain. We cannot decide by it between tubercular and non-tubercular inflammation, but we can between tubercular and non-tubercular tumours. (Ed. *Medical Times and Gazette*, p. 100.)

PHTHISIS.—*Inhalation of Carbolic Acid.*—The inhalation of carbolic acid in fine spray has sometimes a very favourable effect in cases of phthisis, allaying irritation and checking hæmoptysis. Fifteen drops of the pure acid must be dissolved in ʒij. of spirits, and the solution mixed with ℥xxxij. of water. There are various instruments now sold for reducing fluid to a fine spray. (Dr. J. R. Wolfe, p. 108.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

CHOLERA.—Death in cholera results from the loss of the serum of the blood. After, therefore, the feculent discharges have ceased, and the rice-water evacuations commence, prevent the exit of the fluid from the rectum by means of a towel very tightly rolled up and placed longitudinally beneath the buttocks. The towel should be sprinkled with a solution of chloride of lime. The rectum may even be plugged in some cases. A flannel binder encircling the body exerts a wholesome pressure, and it may sprinkled with chloroform to allay the spasms which are sometimes so excruciating. (Dr. John Gason, *Lancet, Dec.* 23, 1865, *p.* 697.)

Dr. Bowerbank, who has had experience of the effects of various modes of treatment of cholera at Jamaica, states that the administration of opiates and stimulants always proved injurious. He saw more good arise from the administration of Stevens' saline powders than from any other mode of treatment. He treated seventeen patients towards the close of the epidemic with iced sea-water, the patients took it greedily and all recovered. In almost every case after the fourth or sixth dose, the alvine dejections and the vomited matters became tinged with bile. (*British Medical Journal, May* 5, 1866, *p.* 470.)

Choleraic Diarrhœa.—When abruptly checked by opium, choleraic diarrhœa often returns immediately the effects of the opium passes off, the patient meanwhile having a hot-skin, quick pulse, and headache, or the patient may pass at once into the stage of collapse. It should be treated by emetics, castor oil, or some other mild laxative, and cold water; not one case out of a large number so treated passed into the stage of collapse. The effect of the opium is highly prejudicial, for it arrests the elimination of the poison through the mucous membrane of the bowels. (Dr. Johnson, *British Medical Journal, May* 12, 1866, *p.* 483.)

CONSTIPATION.—*Atropia.*—Atropia when it has been taken internally for a few days produces slight relaxation of the bowels, and if constipation existed it is removed. The stools, however, are but little altered in character, and the intestinal secretion but slightly increased. This peculiar action of atropia arises from its depriving the mucous membrane of the bowel of its mucous covering, whereby the fæces more readily excite muscular action. The action of atropia differs from that of common purgatives in that its relaxing power is not followed by a disposition to constipation. In cases of simple constipation the following plan of treatment is the best:

℞. Magnesiæ sulphatis ʒj ; acidi sulphurici aromat. ♏ x ; tinct. aurantii ʒj. ; aquæ ad ℥j. M. Let the above draught be given the first thing in the morning, and the last thing at night, on an empty stomach, and let $\frac{1}{60}$ of a grain of atropia be added to the draught at bed-time. (Dr. A. Fleming, p. 110.)

Obstinate Constipation.—Electricity.—When electricity is employed for the relief of obstinate constipation it must be conducted through the rectum, care being taken to employ it gently, but repeatedly, our object being rather to restore the action of the bowel by small but successive doses of the stimulus, instead of attempting to dislodge the impacted contents by one powerful application of the galvanic current. It may be easily applied by passing a copper wire, with a button on the distal extremity, through a rectum tube, the excitor of the other wire being applied to the abdominal walls. Duchenne's galvano-magnetic machine is the best for this purpose. (Dr. A. Fleming, p. 114.)

CHRONIC ENLARGEMENT OF THE LIVER.—Mercury is useless and indeed actually injurious in cases of chronic enlargement of the liver, such as occurs amongst Europeans in the East Indies. The nitro-muriatic acid bath is of great use. An ounce and a half of strong acid must be added to each gallon of water to form a bath. The patient before going into the bath should be covered over with blankets until a gentle perspiration is induced, and when in the bath a covering blanket should be drawn over the head and shoulders to confine the steam and enable the patient to inspire it. The external application of acids act far more quickly and surely than their administration by the mouth. Even in irremediable cases the bath proves useful in cleaning the tongue, improving the appetite, abating thirst, and in many cases in retarding the progress of disease. It is necessary that the bath be made of wood, and it must be no larger than is absolutely needful to contain the person. When the patient is in so weakened a condition as to be unable to bear the immersion or sponging, a swathe damped in the solution must be worn round the body and covered with oiled silk. This may be continued for any length of time, and great advantage is often the result. (Sir J. Ranald Martin, p. 125.)

DIGESTION.—*The influence of dilute Alcohol on.*—Even the most dilute alcohol arrests digestion of animal food until it has been absorbed, digestion then goes on slowly and less perfectly than natural. A little of Allsopp's bitter ale completely arrests the artificial digestion of beef in a solution of gastric juice, which would otherwise completely digest it in eight hours. (Dr. Munroe, p. 365.)

Obstruction of the Bowels.—Inject by means of the long tube of a stomach pump a warm fluid as a solvent or diluent, allow it to remain two or three hours, then reverse the action of the pump and eject, or rather draw out. A very interesting case illustrating the success of this plan of treatment is related. (Dr. E. Fleming, p. 357.)

Phagedenic Ulceration of the Mouth or Soft Palate.—Let the patient inhale a mixture of oxygen and atmospheric air, in the proportion of one part of the former to four of the latter. Rapid improvement generally results. A vulcanised india-rubber bag may be used, with a stop cock and mouth piece. (Dr. R. H. Goolden, p. 49.)

Tænia.—*Turpentine.*—Oil of turpentine, combined with castor oil, three drachms of each, is a better remedy for tænia than oil of male-fern. (Dr. Headland, p. 361.)

AFFECTIONS OF THE URINARY ORGANS.

Acute Orchitis.—*Puncture and Compression by Strapping.*—In cases of acute orchitis after puncturing the testicle through the front or lower part, so as at the same time to let off the serum in the tunica vaginalis, and relieve the tension of the tunica albuginea, as soon as the bleeding has ceased apply a firm case of strapping, put on as tightly as the patient can bear it. A full dose of opium should be given at the same time, and repeated twice or three times. (Dr. W. Spencer Watson, p. 198.)

Ammoniacal Urine.—Ammoniacal urine depends upon decomposition of the urea, owing to inflammatory action in the mucous membrane of the bladder, whilst at the same time the alkalinity reacts upon the bladder, and increases the inflammatory mischief. The urine is usually acid when it first enters the bladder. In the treatment, the main object to be attained is the stoppage of the formation of carbonate of ammonia in the bladder. For this purpose the bladder must be kept as empty as possible by the more or less frequent introduction of the catheter, when this does not produce much irritation. Vegetable acids run off far more freely by the urine than do mineral ones and hence are most suitable to these cases. Citric acid or lemon juice should be given in sufficient doses to keep the urine feebly acid. By washing the bladder with small quantities of hydrochloric or acetic acid and water, after the urine is drawn off with the catheter, the ammonia may be easily neutralised, and a very small quantity of feebly acid fluid may be left in the bladder to counteract the decomposition of the urea, and to make the surface of the bladder itself less foul. (Dr. Bence Jones, p. 132.)

BILE IN THE URINE.—*Detection by Chloroform.*—Place in the test tube forty or fifty grammes of the urine to be examined, and add to it four or five grammes of chloroform, and then shake the mixture. If the urine contains bile it immediately assumes a fine yellow colour, and on allowing it to rest, the chloroform falls to the bottom of the tube, drawing with it the fatty matters of the urine coloured by the yellow biliary substance. (M. Cunisset, p. 365.)

BRIGHT'S DISEASE.—The bowels, skin, and lungs, are more powerful auxiliaries in the elimination of urinary products than is generally supposed. The cutaneous perspiration not only carries off water but many of the organic as well as inorganic urinary salts. Thus in health, it has been found that the sweat contains urea, uric acid, phosphates and chlorides, while in disease it contains even the insoluble oxalate of lime. Urea and urates are also contained in the pulmonary exhalation. The vicarious action of the bowels is to be induced by the internal administration of mild or drastic purgatives. When there is much dropsy elaterium combined with hyoscyamus is a favourite remedy. (Dr. G. Harley, p. 144.)

LITHOTRITY.—Under certain restrictions with regard to the manner of operating, lithotrity is an eminently successful operation, and is attended with very little risk. The following rules should be observed: Let the natural efforts be trusted to as much as possible for the removal of the débris. Do not inject the bladder before operating. Introduce the lithotrite only once at each sitting. Let the lithotrite remain in the bladder only from one minute to a minute and a half at each sitting. It is rarely necessary to use the fenestrated lithotrite more than once at the outset of a case, in dealing even with a very large stone. For subsequent sittings, and for smaller stones, at the outset, the flat-bladed instrument, which is much less irritating to the bladder and pulverizes more completely, should be alone employed. (Mr. H. Thompson, p. 195.)

LITHOTOMY.—*New Instrument for.*—A new instrument has been devised by Mr. Henry Lee for facilitating the operation of lithotomy. It consists of two blades like an ordinary lithotrite, but the lower one is deeply grooved on the right side. It is passed and the stone seized and held between the blades as near the neck of the bladder as possible. The operation is now proceeded with, the groove serving as a guide to the knife. As the groove terminates half an inch from the commencement of the curve the incision is not carried through the prostate, which must be dilated with the finger and pressed back over the stone at the same time that this is pulled forward and removed. The instrument is thus seized before any incision is made, and all delay in the seizure of the stone by the forceps after an opening has been made is avoided. Moreover the instrument itself determines the extent of incision necessary for the size of the stone, for a large stone separates the blades more, and so causes the groove on the under blade to guide the knife deeper. (Mr. H. Lee, p. 197.)

POLYDIPSIA.—*(Diabetes Insipidus.)*—Valerian administered in large and rapidly increasing doses is a most serviceable remedy in diabetes insipidus. A case is related in which the specific gravity of the urine rapidly rose from 1000 to 1006 under large doses of valerian in powder. (Dr. Reith, p. 47.)

TUBULAR NEPHRITIS.—Tartarized antimony has no strong action on the kidneys, and has a very decided action on the skin, hence its applicability to cases of tubular nephritis which generally results from stoppage of the action of the skin from cold, or is accompanied by that condition as following scarlet fever. Digitaline is the only diuretic suitable. As the disease becomes more chronic iodide of potassium may be used in diuretic doses. (Dr. Bence Jones, p. 137.)

AFFECTIONS OF BONES, JOINTS, ETC.

COMPLETE ANCHYLOSIS OF THE KNEE-JOINT.—*Forcible Flexion.*—In a very interesting case of complete anchylosis of the knee-joint from rheumatic arthritis, in which forcible flexion was successfully performed by Dr. P. C. Smyly of Dublin, the points worth noting were as follows: Two strong iron splints well padded with cotton wool were bandaged to the thigh—one in front and one behind—in order to avoid the danger of breaking the thigh bone instead of the knee. Dr. Smyly remarked that he had been present on two occasions when the femur gave way in the middle. After the operation the limb was placed in a long-jointed splint with a screw behind, and at an angle of about forty-five degrees. The knee was kept constantly iced for forty-eight hours and the foot kept warm. Every day the position was slightly changed so that towards the end of a week the limb was again straight. For about six weeks a gradual process of flexion and extension was persevered in, the splint was then removed. (Dr. P. C. Smyly, p. 162.)

DISLOCATIONS OF THE SHOULDER-JOINT.—*To Reduce.*—Kneel behind the patient and place the palm of one hand firmly on the scapula and the fingers in the axilla under the head of the humerus. With the other hand grasp the condyles and press the arm to the side, then draw it backwards, and thus get a better hold with the fingers engaged at the head of the bone. By the next movement slide the arm forwards and depress the elbow, at the same time rotating outwards and lifting the head of the bone into its place. By this plan many cases may be successfully reduced; but when the patient is very muscular it is better to adopt the usual plan, with the heel in the axilla. (Mr. W. E. Porter, p. 164.)

FRACTURES OF THE LEG.—*New Form of Cradle.*—A new form of cradle for suspending the splinted leg in cases of severe fracture, invented by Mr. Henry Greenway, of Plymouth, is described at p. 172. It appears likely to conduce greatly to the comfort of the patient.

FRACTURE OF THE RADIUS AT THE STYLOID PROCESS.—The ordinary straight splints are not satisfactory for the treatment of this fracture owing to the radius not being a straight bone. Its concavity at the wrist should be maintained by any splint adapted to it. The only splint which answers this end is that known as Gordon's splint, a description of which will be found at page 166. (Mr. L. Tait, p. 165.)

INJURIES TO THE HEAD AND NECK OF THE HUMERUS.—Injuries to the head and neck of the humerus are divisible into those *below* the tuberosities and those *through* the tuberosities. In the latter case there are all the symptoms of dislocation into the axilla *a few months after the accident*, for the upper fragment is dragged upwards with the fractured surface pointing outwards, owing to the action of the muscles inserted into the tuberosities, and the form of the neck being thus altered the head is gradually displaced downwards in the axilla. It is necessary to forewarn a patient of this. The only plan likely to prevent this is to carry the patient's arm out, at right angles to the body. This would prove very inconvenient in practice, but may be tried if the use of a long straight splint, well padded, and pressed into the axilla, does not sufficiently prevent displacement. (Mr. J. Hutchinson, p. 168.)

SPLINTS.—*Gutta Percha.*—No matter in what form the gutta-percha is, nor in how many small threads or large balls, place it in a vessel of water, and boil it till it is perfectly softened throughout; now collect it into one ball; dip it in cold water to enable it to be handled without burning, and knead it into a uniform mass with the hands. To do this, if it is very good and tough, will require some strength and also warming two or three times: but eventually it will form a uniform plastic mass not much harder than stiff putty. When in this state, place it on any flat surface which is wet, and with a roller—a common glass rolling-pin dipped in water is the best—roll it into a sheet of the desired thickness, let the roller slide upon it in all directions, and continue this action till the surface is uniform; it will now be fit for cutting into shape. This can readily be done either when hot or cold with a pair of scissors, and no force is required to mould it into the form of splint required, the only precaution requisite being to hold it steadily in that shape till it is cold; after which it will not alter unless again made hot. It may sometimes be requisite to join two pieces of gutta-percha together. This is readily done by warming the two surfaces to be united by dry heat, with a hot iron, not burning, but merely softening the surface. Two such hot, dry, soft, clean surfaces will unite most perfectly when brought into contact with each other, if held together till they are cold. It may sometimes also be useful to know how two pieces of this material may be moulded on to each other without sticking. If one be cold and the other hot, they will not unite, especially if the cold surface be anointed with soap-suds. (Mr. E. Truman, p. 176.)

Leather-Felt Splints.—These simple and admirable splints consist of stout felt, lined on one side with wash-leather. All that is necessary for their application is to place a splint (with the leather side downwards) upon a table, then paint the felt side with a liquid which is supplied by the inventor (Mr. J. G. Hides, of Mortimer-street), then apply the splints to the injured limb, and bandage with an ordinary roller, leaving a little interval between each turn of the bandage. The splint hardens like a piece of wood in about thirty-five minutes. These splints are extremely light and are capable of adaptation to any part without padding. They are about the same price as wood splints, and can be used again and again by merely painting the felt side afresh with a weaker solution of the liquid. (p. 175.)

SUBACUTE SYNOVITIS.—*Effusion from.*—After all inflammatory symptoms have diminished, if the amount of effusion shows no tendency to diminish, apply a large blister to the joint, and dress with mercurial ointment, or if the blistered surface shows a tendency to heal rapidly, with equal parts of mercurial and savin ointment. (Mr. W. Adams, p. 163.)

AFFECTIONS OF THE SKIN, ETC.

Chilblains.—Moisten a small piece of linen with the following, and apply it to the part, or it may be rubbed in occasionally: Balsam of Peru ʒss.; rectified spirit ℥iss. Dissolve, and add hydrochloric acid ʒss.; compound tincture of benzoin ℥ss., mix. When the chilblain is broken calamine ointment is a very good dressing. (Dr. H. S. Purdon, p. 201.)

Eczema Infantum.—Give minute doses of arsenic, and one grain of calomel twice a week at bed-time, unless active diarrhœa be present. Apply lint very thickly spread with zinc ointment, and allow the ointment to remain on for a week, when it may be removed with a spatula: a nearly healthy surface will then be exposed. (Mr. Erasmus Wilson, p. 203.)

Erysipelas.—*Warm versus Cold Applications.*—Never use cold applications in cases of erysipelas. Warm fomentations medicated with poppy or hemlock should be used. The vitality of the part in which erysipelas occurs is always lowered. (Mr. J. Hilton, p. 45.)

Gangrene, Suppurations, Sloughing, &c.—*External use of Sulphites.*—In all external wounds, sloughing, gangrene, and ulcerations, the application of the sulphites or hyposulphites of soda or potass are useful in destroying any putrefactive tendency, and exciting healthy action. One part of sulphite of soda may be dissolved in five or ten of water, and frequently used as a lotion, or on compresses of wet lint. (Dr. Polli, p. 334.)

Local Pains of the Walls of the Chest.—Chlorocarbon, whilst it possesses the anæsthetic properties of chloroform has less of the local irritating effects of the latter. Ten or twenty drops injected subcutaneously will relieve pains of the walls of the chest or abdomen, without producing the nausea so frequently the result of the hypodermic injection of preparations of opium and morphia. (Sir J. Y. Simpson, *Medical Times and Gazette, Dec.* 16, 1865, *p.* 652.)

Pruritus Senilis.—A case is related in which the subcutaneous injection of one fourth of a grain of morphia, repeated in two days, completely cured the intolerable itching. In another case one injection produced the same result. (Dr. H. Beigel, p. 202.)

Wounds.—*Chloride of Zinc Lotion.*—The discharge from a wound, whether the result of accident or surgical operation usually decomposes whenever covered for even a few hours, as evidenced by the peculiar sickly smell. If, however, the wound is well bathed with a lotion of twenty grains of chloride of zinc to the ounce of water this is prevented, and the wound will frequently heal by the first intention, without swelling, pain, or discoloration. This plan has been tried in a variety of operations, the removal of tumours, amputations, even with extensive and thin flaps, operations about the rectum, involving the mucous membrane, and in the perineum. In many cases the wounds have healed in twenty-four hours, without fulness or swelling, and leaving a line of cicatrix which after a short time could hardly be seen or felt. (Mr. C. de Morgan, p. 157.)

VENEREAL AFFECTIONS.

GONORRHŒA AND GLEET.—*Soluble Bougies.*—Soluble bougies, two or three or more inches in length and made of cacao butter may be impregnated with nitrate of silver, (a quarter of a grain) or tannin, or acetate of lead, and being introduced into the urethra in cases of chronic gonorrhœa or gleet, form an excellent means of thoroughly applying remedies to the mucous membrane. They may also be made sedative by impregnating them with opium or belladonna. It is probable that many of these cases are so troublesome because the remedies applied are not allowed to remain sufficiently long in contact with the mucous membrane. (Mr. Henry Thompson, *Lancet, May* 12, 1866, *p.* 513.)

SYPHILIS.—*Calomel Vapour Bath v. Syphilization.*—The treatment of a case of constitutional syphilis by syphilization takes on an average nineteen weeks, that by the calomel vapour bath takes five weeks and a half. It cannot be urged against the use of the calomel vapour bath that it is unsuccessful, for no case occurs of severe complication of the deeper structures where the calomel bath has been properly used for primary syphilis. The time required for the treatment of syphilis by means of syphilization is so long, and the inconveniences attending it is so great that it is not likely to be adopted in private practice in England. The calomel vapour bath does not act effectually, unless a hot-air or vapour bath be first taken so as to ensure a supple and absorbing surface. (Mr. H. Lee, p. 228.)

AFFECTIONS OF THE EYE AND EAR.

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Atropine Pencils.—Pencils or points impregnated with atropine and adaptable into wooden holders, which may be carried about in the pocket are by far the most convenient mode of using atropine to dilate the pupil. If the atropine point is lightly passed, once or twice, over the conjunctiva of the lower eyelid a sufficient quantity of atropine is washed off to dilate the pupil for ophthalmoscopic or other purposes. These points may be obtained from Mr. Curtis, of Baker-street, London. (Mr. J. Z. Laurence, p. 212.)

BLACK CATARACT.—There is a form of cataract in which to the naked eye all appears healthy, but by the ophthalmoscope it is seen that the lens is of a deep brown or black colour. The sight is lost. These cases are rare. It is of course impossible to illuminate the interior of the eye. (Mr. Haynes Walton, p. 209.)

BLUE GLASSES.—Blue tinted glasses are on the authority of Von Graefe and Professor Böhm superior to neutral tint as a means of modifying the intensity of light. (p. 212.)

FUNDUS OCULI.—*To Examine without the Mirror of Ophthalmoscope.*—Place the lamp on a table in a darkened room and let the patient sit on a chair two or three feet from the table, and facing the light. If the pupil is well dilated with atropine the fundus is seen to be illuminated, the Surgeon sitting with his back to the light. A lens of about two inches focus is necessary to see the details of the posterior internal surface of the eye. (Dr. Roseburgh, p. 204.)

GONORRHŒAL OPHTHALMIA IN CHILDREN.—Wash away the discharge every half-hour with lukewarm water, and apply within the lids a solution of alum of the strength of three grains to the ounce. The diluted nitrate of silver stick is also very useful. The nitrate is diluted with nitrate of potash. (Mr. Ernest Hart, p. 208.)

GRANULAR LIDS.—*Pressure.*—If pressure is applied to the granular lids without pressing on the globe of the eye we frequently gain much benefit in these cases. This may readily be done by means of an instrument consisting of two ivory plates, one fitting into the concavity of the under surface of the lid and the other upon the convex upper surface: the upper plate can be approximated to the under by a screw, and the amount of pressure delicately adjusted. The lid compressor may at first be applied for one hour daily, and if well borne, for two, three, or four hours. One great advantage which this treatment has over the ordinary methods of scarification and subsequent application of various caustics, is that there is nothing in pressure which can tend to the production of entropium with trichiasis. (Dr. W. Stokes, Jun., p. 204.)

IRITIS.—*Treatment of by Atropine.*—One half of cases of iritis, whether syphilitic or not, can be cured by the application of atropine alone. A little solution of the strength of two grains to the ounce should be dropped into the eye six times, at intervals of five minutes in the morning, and six times also in the evening. On the following day, if the pain is lessened and the pupil is beginning to dilate the case is slight, and atropine alone will cure it. If the pupil is affected, and the symptoms are unabated, mercurial treatment must be commenced without delay. Mercury acts much better on the system when it is introduced through the skin than by the digestive organs. The patient should be confined to bed, and a broad piece of flannel well smeared with mercurial ointment wrapped round each arm. The gums generally become affected in from three to four days. It is not necessary to rub in the ointment. Atropine should be used during the whole period of treatment, except where it causes great pain or increases conjunctival irritation, in which case it may be temporarily suspended. (Mr. T. Pridgin Teale, jun., *Ophthalmic Hospital Reports, Vol.* v., *Part* 2. *p.* 156.

MALIGNANT AND RECURRENT TUMOURS OF THE ORBIT.—After removing the tumour with all the tissues as far back as the optic foramen, apply chloride of zinc paste spread on strips of lint to the bottom and sides of the orbit, so as to effectually destroy any germ of the disease whch may have escaped the knife. The actual cautery may be applied to the periosteum, in spots where there appear to be small adherent particles of the disease which it is difficult to get away with the knife. (Mr. De Morgan, Mr. Hulke, Mr. Lawson, p. 213.)

SPECULUM AURIS.—*(Dr. Brunton's.)*—In examining the membrana tympana with the ordinary speculum there is frequently great difficulty in seeing owing to the observer's head obstructing the light, and the eye cannot be brought near enough the object to permit of minute examination. All this may be obviated by the use of an instrument on the principle of the endoscope, the light being admitted at the side. The following simple instrument is well adapted to the purpose. The instrument consists of a brass tube, two inches long and three quarters of an inch in diameter, to one end of which is made to fit on, by sliding, an ear-piece similar to Toynbee's aural speculum. At the other end is an eye-piece with a lens of moderately magnifying power; the eyepiece slides so as to admit of focal arrangement to suit the eye of the observer. In the body of the instrument, near the ear-piece end, and set at an angle of 45°, is a concave mirror with a hole in the centre; this aperture in the mirror is in the line of the axis of the tube and ear-piece. At a right angle to the body of the instrument, and opposite to this mirror, is adapted a sliding funnel-shaped polished silver reflector for collecting and concentrating the rays of light, so that the rays are let in at the side, and, falling on the mirror are reflected and concentrated into the ear, and carried back to the eye of the observer through the hole in the mirror, and are magnified by the lens of the eye-piece. There is also a handle attached to the light-reflector, which works on a sliding band, and can be turned about to suit either hand of the observer as he may wish, when looking at the right or left ear. With this instrument the smallest change of structure may be noted, and the movement of the membrane readily observed. The blood-vessels that traverse the membrane can be seen in the healthy ear. (Dr. J. Brunton, p. 214.)

Tympanum.—*Application of Pulverised Fluids to.*—By means of a specially contrived instrument pulverised fluids can be applied directly to the interior of the tympanic cavity, and the same instrument serves for the application of such agents as carbolic acid and creosote to nasal cavities in cases of ozœna. The apparatus consists of a small graduated glass syringe similar to that used for subcutaneous injection. The solution is gently forced guttatim into a cylinder, and at the point where it leaves the nozzle of the syringe it is caught by a current of air sent by a pump worked with a proper degree of force by the hand. This drives the fluid forward in a pulverised state. This small apparatus is then attached to an ordinary Eustachian catheter previously introduced, and suspended by a suitable apparatus. To ensure complete pulverisation, the end of the catheter is covered by fine gauze wire. (Dr. E. Bishop, p. 216.)

MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

Chlorocarbon in Midwifery.—Chlorocarbon will probably prove especially useful in midwifery, for it abolishes pain without affecting consciousness. It is during its early stage of action a powerful stimulant to the circulatory system. (Dr. Sansom, *Lancet, March* 17, 1866, *p.* 288.)

Chlorosis.—*Iron.*—Iron is useful in chlorosis only because of a peculiar influence which causes the food to be transformed more easily into blood. Other metals which are not normal constituents of blood globules, such as manganese and silver, will sometimes act even better than iron. There is immense value in a purely hygienic means in the treatment of chlorosis. (Dr. Brown-Séquard, p. 314.)

Difficult Turning.—By the use of the following instrument for applying a running noose of tape round the ankle of the child when very high up, turning may be accomplished in cases where embryotomy apparently was the only method practicable. The instrument is a staff of German silver, with a horse-shoe shaped bi-valvular tube, with open bevelled extremities fixed to its top. At the handle is a spring, by means of which the horse-shoe shaped tube can be opened into two semi-circular grooved valves. A loop of the running noose of tape is put into, and fastened in, the bi-valvular tube. The instrument is then introduced, with the horse-shoe along the arm and hand of the accoucheur, up to the foot, which is then slipped into the circle formed by the tube and the free part of the loop, between the two extremities of the horse-shoe. By pressing on the spring the tube readily opens, and lets the loop of the running noose free, surrounding the child's ankle. It is then tightened by pulling on the free ends of the noose. (Dr. Hyernaux, p. 247.)

Displacement of the Uterus.—Take a circle of gutta-percha and enclose in it a copper wire of No. 12 thickness. A stem of the same wire, a continuation of that enclosed in the gutta-percha, and bent to the curve of the vagina, must be brought forwards to the upper edge of the mons veneris. The os and cervix must be placed in the circle, which must be deep enough to contain them. At the upper edge of the mons veneris the wire must be bent into two semi-circular horns, one to run along the lower part of the abdomen on either side, immediately above the groins. These horns should be fastened to the abdominal bandage by tapes or stout pins. The instrument is now secure, and invariably affords relief without giving rise to any pain or irritation. It requires a little modification in the shape of the gutta-percha cup according as the case is one of anteversion or retroversion. Its cost is not more than threepence. (Dr. Main, p. 275.)

Extreme Flexions of the Uterus.—A description of a new instrument for the relief of extreme flexions of the uterus, invented by Dr. Wright, of the Samaritan Hospital, will be found at p. 279.

Glandular Tumour of Mamma, with Cysts.—*Injection with Iodine.*—The usual practice in these cases is to remove the tumour or to lay it open and stuff with lint. Instead of that introduce the needle of a subcutaneous injection syringe and withdraw the fluid through it, then inject with tincture of iodine. The indurated gland around the cysts disappears when the cysts are destroyed. (Mr. W. Martin Coates, p. 315.)

HEMORRHAGE AFTER LABOUR.—*Injection of the Uterus with Perchloride of Iron.*—After clearing out the uterine cavity of placental remains and clots, inject a solution of perchloride of iron into the uterus. (Dr. R. Barnes, p. 245.)

Hemorrhage from Sub-involution of the Uterus after Abortion or Labour.—There is not the least danger in injecting the uterus with styptic solutions such as a solution of perchloride of iron, or in swabbing the interior by means of a piece of sponge. It is necessary, however, that the internal os should be first freely dilated. We must rely greatly upon the improvement of the general health which is always more or less impaired. (Dr. R. Barnes, p. 252.)

HYSTERALGIA.—*Chlorocarbon Vapour in.*—In cases of hysteralgia the injection of the vapour of chlorocarbon sometimes relieves the pain. The first patient in whose case this was tried said that she experienced so much relief that she slept the following night "far better than she had done for weeks." The best apparatus is a common enema syringe with the nozzle introduced into the vagina, and the other extremity placed in a phial containing the chlorocarbon. (Sir. J. Y. Simpson, *Med. Times and Gazette, Dec.* 11, 1865, *p.* 651.)

LIEBIG'S FOOD FOR CHILDREN.—In the preparation of this food the malt should be well freed from husk and finely ground, and the wheat flour should be slightly baked, in order to partially cook the starch entering into its composition. A thermometer should also be employed in the preparation of the food, as a temperature ranging between 140° and 148° is amply sufficient to effect the complete transformation and solution of the starch corpuscles, and, indeed, to cook the food sufficiently. (Dr. A. H. Hassall, p. 326.)

MECHANICAL DYSMENORRHŒA.—The seat of obstruction is almost invariably at the os externum. The obstruction is due chiefly to the small round os itself; partly to the pointed elongated form of the lower part of the vaginal portion, and partly to an unusual rigidity of structure. The vaginal portion is in fact in excess, and is subject to flexions. These flexions, which may be either anterior, posterior, or lateral, are flexions of the cervix and not of the body, and the angulation sometimes produces stricture. Any plan of treatment which is not adapted to remove the mechanical cause is not likely to do more than mitigate the symptoms. Neither

Simpson nor Marion Sims have found dilatation either safe or successful, and it is liable to be followed by ill consequences, such as pyæmia and peritonitis. The instrument used by Dr. Barnes is contrived to cut like a pair of scissors. One blade, strong, and somewhat probe-shaped, passes through the narrow os into the cervix, whilst the other blade, slightly concave on the cutting edge, so as better to seize the rounded surface of the cervix, is applied at any part of the vaginal-portion the operator chooses. Then the part to be divided being exactly included between the blades is cut through. Thus we are sure of cutting as much as we want, and no more. There is this great advantage in cutting from without inwards, instead of from within outwards, that we are cutting towards safety and not towards danger. (Dr. R. Barnes, p. 301.)

Ovarian Tumours.—*Chlorate of Potash.*—Four cases of ovarian disease are related in which the internal administration of chlorate of potash produced a most marked effect in causing lessening of the tumour; two were quite cured, the tumour altogether disappearing. A dessert-spoonful of a saturated solution of the salt was given thrice a day. (Mr. W. Craig, p. 296.)

Ovariotomy.—In a case of ovariotomy related by Dr. Aveling, of Sheffield, the pedicle which was short and about four inches broad, was transfixed with a needle carrying a double thread. The two ends of each ligature were then brought up together on either side, and drawn through two iron wire coils; each coil had fitted upon its end a cross-bar, and round these cross-bars the ligatures were secured after they had been drawn sufficiently tight to compress the vessels. The pedicle was then placed in the hook of Dr. Aveling's polyptrite and crushed through. As there was no hemorrhage it was returned at once in situ, and the wound closed with iron wire sutures. The heads of the two coil clamps were left protruding from the lower angle of the incision. In two days the ligatures were unwrapped from the cross-bars, the coils removed, and the ligatures drawn out, a good recovery followed. The coil clamp will especially be found of use where the pedicle is short. (Dr. J. H. Aveling, p. 294.)

Polypus Uteri.—It is not correct to say that there is no danger of hemorrhage in cases of excision of a uterine polypus, for some are fed by an artery which may be as large as the radial. It is better in all cases where it can be applied, to remove a polypus by the ecraseur, rather than by the knife or scissors. If, however, from the large size of the polypus, and the contracted state of the vagina this is impossible, the ligature with excision, or the ligature alone must be employed. (Dr. J. G. Swayne, p. 270.)

Procidentia Uteri.—A descent of the uterus is invariably preceded by a prolapse of the anterior wall of the vagina, which draws down the cervix. The ordinary perineal operation has no effect in providing support where it is wanted, namely, anteriorly, at the connection of the cervix uteri with the base of the bladder. The redundant tissue of the anterior wall must be removed. Raise the hypertrophied vaginal tissue by a tenaculum and remove it, afterwards bringing the edges together by silver wire sutures passed transversely. (Dr. Marion Sims, p. 255.)

Prolapsus Uteri.—*Plastic Operation.*—Most of the failures which have occurred in operating for the permanent cure of prolapsus uteri have arisen from denuding the vagina of mucous membrane in a pyramidal form, of which the apex is above. Just the opposite should be done. *The base should be above*, because here should occur the greatest resistance to the descent of the womb. It is always well to lay bare an amount of surface somewhat in excess of that which we really wish to adhere together, because union by the first intention never takes places on the edges adjoining the mucous membrane, however carefully they are brought together. The denuded surfaces are better kept together by flat ribbons of silver, one sixteenth of an inch in breadth, and very thin and soft; of course the needle to carry this requires a transverse slit. Instead of the two pieces of quill or bougie ordinarily used, two pieces of stout silver wire must be soldered together at the ends; when the end of the flat ribbon is passed through the slit between these wires it can be fastened at once by giving it two or three twists. One of these double wires of course is required for either side. It is unnecessary as a rule to divide the sphincter. A firm bandage with a perineal pad should be worn for two or three months after the operation, until the parts are consolidated. (Mr. R. Ellis, p. 260.)

Caustic Applications.—In a case of prolapsus uteri in a patient 72 years of age, Dr. Routh painted the strong nitric acid over a portion of the external skin, which was very loose, and over the mucous membrane of the vagina, about one and a half inches upwards, and for the lower half of the external opening. The pain was considerable at first, but subsided entirely after four hours. The sloughs separated on the eleventh day. The granulations were well washed by a stream of warm water, the parts were brought together by clamps, and two deep sutures applied. A catheter was left in the bladder. The case did well. (Dr. C. H. F. Routh, p. 264.)

Puerperal Albuminuria.—Puerperal convulsions are not generally the result of the true albuminuria of pregnancy, in reality they are much more frequently the concomitants of true kidney disease assisted by the effects of the puerperal condition. While in the albuminuria of pregnancy, unless the symptoms be severe, the case may almost be left to nature, in pregnancy associated with true kidney disease energetic treatment is demanded, even to the induction of premature labour. Pregnancy in many cases ultimately induces true kidney disease by keeping up renal congestion. (Dr. G. Harley, p. 142.)

Ranula.—*Injection of Iodine.*—Inject fifteen minims of compound tincture of iodine into the interior of the tumour. (Mr. W. M. Coates, p. 316.)

Relaxation of the Pelvic Articulations.—At the close of pregnancy and after labour the pelvic articulations are sometimes so relaxed as to prevent the patient taking walking exercise without considerable pain in the back and pelvis. The gait produced is peculiar, being a species of waddling, each leg being advanced with difficulty. There is always great accompanying debility. This state is but an exaggeration of a physiological fact, the object of which is to facilitate the passage of the fœtus. A laced jean belt must be constructed, to support the pelvis and hips. Before, however, this is done, it is well to try the effect of a circular bandage tightly applied round the hips. If the patient is able to walk with ease, the belt may be made for more permanent wear. (M. Trousseau, p. 320.)

Spina Bifida.—*Injection of Iodine.*—In a case in which the opening into the spinal column was about half-an-inch long and a quarter wide, the fluid was withdrawn by means of a subcutaneous injection syringe, the needle of which was passed first through healthy parts before reaching the tumour; a solution of iodine ten grains, iodide of potassium twenty grains, and water an ounce was then injected, and the tumour painted over at once, and daily afterwards, with contractile collodion. A cure resulted. (Mr. W. M. Coates, p. 317.)

Uterine Dilator.—A new uterine dilator, intended to act more on the internal os and upper part of the canal, than any instrument now in use will be found described and figured at page 286. (Mr. J. W. Hembrough.)

Uterine Leucorrhœa.—*Injection of Tincture of Iodine.*—Having by the introduction of a short-haired brush removed the layer of mucus adhering to the interior of the cervix and uterine cavity, inject by means of a gum elastic catheter a lotion consisting of one part of the compound tincture of iodine to two parts of water. (Dr. Murray, p. 300.)

Vaginal Rectocele.—Vaginal rectocele is protrusion of the dilated rectum and posterior wall of the vagina through the the relaxed vaginal outlet. There is no displacement of the entire rectum, the anterior part only is involved. It is remediable by a plastic operation, whereby the relaxed state of the vagina is remedied. The operation necessary is essentially similar to the one for ruptured perineum. (Mr. J. R. Lane, p. 287.)

MISCELLANEA.

Acids.—The rule is sulphuric acid to astringe, hydrochloric acid to promote digestion, and nitric acid to promote secretion. (Dr. Bence Jones, *Lancet, Dec.* 9, 1865, *p.* 641.)

Anæsthesia.—*Anæsthesia by Mixed Vapours.*—Chloroform is the wrong thing to begin with, it is a depressant to the heart. We ought to commence the anæsthetic operation with a gentle cardiac stimulant. Let the patient at first inhale a mixture of ether and alcohol vapour, and when the heart and nervous system have fully experienced the power of this excitant, let chloroform vapour be gradually inhaled. This may be readily done by an inhaler specially contrived by Mr. Robert Ellis, of the Chelsea and Belgrave dispensary, for the purpose, described and figured at page 389. By it the relative proportion of these vapours can be regulated with the utmost nicety. Pure absolute ether and absolute alcohol must be used, perfectly free from the abominable contamination of methyl and its compounds. (Mr. R. Ellis, p. 386.)

Chloroform.—In cases of threatened mischief during the administration of chloroform, nothing does more good than pulling forward the tongue, whereby the epiglottis is likewise pulled forward and air admitted into the larynx. Chloroform should never be given to a patient in the sitting posture, or fatal fainting may ensue. (Prof. Spence, Dr. Gillespie, *Edinburgh Medical Journal, March* 1866, *p.* 857.)

Ether and Chloroform.—If, in any case, chloroform seems to produce a depressing effect, it is better at once to substitute ether for it; and in prolonged operations, when anæsthesia has been fairly produced by chloroform, it is a good plan to keep it up by ether. The substitution is never attended with any return of sensation. There seems to be a natural antagonism between these agents. (Dr. Sanson, *Lancet, March* 17, 1866, *p.* 288.)

Local Anæsthesia—Dr. Richardson's Ether-Spray Method.—If ether be forced through a jet by mechanical force, along with a certain amount of air, such intense cold is produced as readily to freeze water exposed to it in a test-tube. The apparatus used (figured at page 372) consists of a graduated bottle for holding ether, through the perforated cork of which a double tube is inserted, one extremity of the inner part of which reaches to the bottom of the bottle. The air is forced into the bottle by means of a compressible india-rubber bellows, and forces the ether through the inner tube, and in fine spray from the terminal jet; a current of air is also forced through the outer tube and terminal jet, which is of course a little larger than, and surrounding the inner, or ether jet. When the spray thus produced is directed upon the skin, insensibility is produced within a minute, and after the division of the skin the ether begins to exert on the nervous filaments the double action of cold and of etherisation, so that the narcotism can be extended deeply to any desired extent. The spray directed upon a half-inch test tube containing water will produce a column of ice in two minutes at most. The spray may even be directed into the bladder, or into the uterus. It may readily be directed upon the gum previous to the extraction of a tooth, rendering the operation quite painless. It is likely to be useful also in a great number of minor operations, such as tying nævus, tying piles, incising carbuncles, opening abscesses, and removing small tumours. In the extreme rapidity of the action of this deadening process lies its safety, suspending life without causing destruction of vitality. Many other fluids have been used instead of absolute ether, of specific gravity 0.720, such as methylic ether, amylene, and kerosolene, but none answer so well; kerosolene is a very impure and unpleasant liquid. The peculiar hardness of the skin which occurs when the freezing mixture of ice and salt is applied, does not occur under the use of the ether spray. Tests for the purity of ether will be found at p. 375. (Dr. B. W. Richardson, p. 369.)

According to the nature of the case the surgeon may either produce a superficial anæsthesia or entire blanching of the part: to produce the latter absolute ether must be used, and the spray directed in a brisk current at a distance of about an inch from the part; to induce the former, or superficial anæsthesia only, a mixture of ether and chloroform, in the proportions seven to one, or six to two, may be used. For opening an abscess, incising a small carbuncle, tying a nævus, or removing very small tumours, for applying nitric acid, and for operations of a similar kind, the mixture of ether and chloroform answers every requirement. In an operation for hernia it would also be better to use the mixture, because the tissues would not be rendered hard, and the dissections could be carried on with delicacy. For teeth extraction the pure ether answers best, it acts rapidly and deeply, and there is no great accumulation of fluid in the mouth. By practice, the two degrees of action may be obtained by ether alone, by regulating the distance from which the spray is directed. By removing the jet three inches from the part a moderate effect is produced. (Dr. B. W. Richardson, p. 380.)

Rhigolene.—It will probably be found that rhigolene, prepared from naphtha, will be the best means of freezing parts by Dr. Richardson's spray method. Common ether will boil at about 96°, while rhigolene boils at 70°. It will be both more effectual and much cheaper than ether. (Dr. H. J. Bigelow, Massachusetts, p. 342.)

Local Anæsthesia.—Certain substances such as atropine, strychnine, morphine, and aconite, when mixed with chloroform and applied to the skin are absorbed very rapidly, while if alcohol is used absorption is very slow. If this by further experience proves correct, we may produce local anæsthesia without subcutaneous injection. (Dr. Brown-Sequard, p. 364.)

HASSALL'S FLOUR OF MEAT.—This food consists, in the concentrated form and with the exception of the water, of the whole of the constituents of the meat, previously freed from bone and visible fat. It is in a very fine powder, finer indeed than flour. A spoonful of it added to ordinary beef tea or soup greatly enhances its nutritive qualities. When taken as a sandwich, or sprinkled over or mixed up with vegetables it is often retained by the stomach when solid meat could not be taken. It is most serviceable in cases of dyspepsia. (Dr. A. H. Hassall, p. 338.)

MILK.—*To Prevent its turning Sour.*—Add to each gallon of milk a teaspoonful of a saturated solution of sulphite of soda. Even in hot weather the milk will remain sweet for a week or ten days, and the sulphite added is quite imperceptible. (Dr. J. Scoffern, p. 341.)

POISONING BY PRUSSIC ACID.—Give the patient one or two drachms of magnesia made into an emulsion with water, and then give, in water, a solution of sixteen minims of perchloride of iron and twelve grains and a half of green vitriol. A harmless Prussian blue results. (Messrs. T. and H. Smith, p. 365.)

PREPARATIONS OF IRON.—Preparations of iron promote the process of oxidation in the body, changing the oxygen taken into the blood by respiration into ozone. They therefore favour the nutrition and activity of the body, and are contraindicated when the process of oxidation is already too energetic, as in inflammatory diseases, or when an increased flow of blood is apprehended. (Dr. A. Sasse, p. 361.)

Purification of Water.—Black oxide of iron possesses a remarkable power of separating the organic impurities from water. The best way of preparing it for this purpose is by heating together hæmatite, or red oxide of iron ore, with saw-dust. The oxide as made by this process contains a small percentage of carbon which, however, renders the substance harder and less brittle. If river water containing much organic impurity be made to percolate a layer of this substance a few inches thick it will loose almost all organic impurity, and be perfectly pure both to taste and smell. It is an admirable substance to use in filters, and, moreover, does not lose its power in the least by time. (Mr. Thomas Spencer, *Med. Times and Gazette, April* 28, 1866, *p.* 447.)

Thermometer.—*Uses of in Diagnosis*—The thermometer is now in constant use as an aid in the diagnosis of certain cases. A set of two, specially graduated for the use of medical men, can be obtained. A persistent rise of the temperature of the body above 99·5° or depression below 97·3° is a sure sign of disease. The temperature is lowered after a full meal and the use of alcohol; it rises as digestion advances. An increase of temperature of one degree above 98° generally corresponds with an increase of ten beats of the pulse per minute. There are however many exceptions to this rule. The temperature is the only certain means of diagnosis between the frequency of pulse produced by weakness and that caused by fever, and by it we diagnose phthisis long before any physical signs can be detected, and can distinguish it from diseases which closely resemble it, as carcinoma of the lung, aortic aneurism, and dilated bronchi. We may also diagnose tubercular meningitis from cerebral tumour. (Dr. F. W. Gibson, p. 20.)

Sick Rooms.—*Disinfection of.*—For a sick room charged with an organic poison, all disinfectants that may be used should belong to the class that assist oxidation. Ventilation and a regular temperature (60° to 63°) increase their action. The three agents suitable are chlorine, ozone, and iodine. Chlorine should not be exposed freely to the air, but the solution should be contained in small stoppered bottles which may be emptied into the room at regular intervals at as great a distance as possible from the patient. When a slow and continuous process of deodorising is required iodine maintains an excellent effect, and if required for more active service it may be evaporated from a heated plate. Ozone, however, is extremely easy of production by means of phosphorus half covered with water, and maintains the air of a sick chamber perfectly sweet. (Dr. T. H. Barker, *British Med. Journal, Jan.* 6, 1866, *p.* 13.)

SINAPINE TISSUE PAPER.—Sinapine tissue paper is a thin glazed paper impregnated with something of a very pungent character, which, when wetted with cold water and applied to the skin, produces in about ten minutes a good deal of superficial irritation. Its effects are more persistent than those of an ordinary mustard poultice, and it is much more cleanly. (Mr. Cooper, p. 363.)

SULPHITES AND HYPOSULPHITES.—*Administration of.*—The salts of sulphurous and hyposulphurous acids with potass, soda, and magnesia, are those most suitable for medicinal use. They are not changed when taken into the stomach, but pass unchanged into the blood and appear in the urine, saliva, and sweat. Hence their use in arresting putrefactive zymotic changes in the body. Their beneficial action is checked by any of the vegetable acids, hence food or drinks containing them must be avoided at the same time. The sulphites are better than the hyposulphites when a rapid curative action is required, but the latter may be advantageously substituted for the former when prophylaxis is aimed at. Sulphite of magnesia has the least taste and is the most active, but being less soluble than the others should be administered in powder. (Dr. Polli, p. 332.)

WHEAT PHOSPHATES.—There is something essentially special in the organized phosphates, those, in fact, which have been found by passing through a living organism as compared with those artificially prepared. It is not the amount, but the kind exhibited which produces the good result. In infant's food, and in our bread and flour, the organised phosphates and cerealin (which has a somewhat similar action to pepsin) have been deliberately rejected. These may be administered medicinally to children and infants where the assimilative function is at fault. In eruptive diseases of the scalp (which are generally associated with faulty assimilation) in rickets, marasmus, chronic diarrhœa, and impaired nutrition of all kinds, the wheat phosphates act marvellously. Pallid children pick up tone, colour, and flesh, worms disappear, the secretions become healthy, and disease goes. They are prepared by carefully evaporating in a water bath a decoction of well-selected bran, mixing the residue with sugar, and reducing to powder. (Dr. Tilbury Fox, p. 130.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—ON FEVERS.

By Dr. H. Bence Jones, F.R.S.

[Perhaps the term zymotic disease is as good as any to designate the action of those poisons which get into the system in such a mysterious way that we cannot explain them. The term implies some action by which different vegetable or animal ferments, living or dead, can be introduced into the body and produce different diseases, such as small-pox, scarlet fever, measles, typhus fever, &c.]

Of all the modified peroxidations that can occur in the body, small-pox is the most definite, because the poison can be got apart, and the quantity necessary for producing the action can be fixed, and through the most glorious discovery of vaccination it can be set in action whenever we please. We can almost see it passing from the cellular tissue into the blood, and from the blood into every particle of every texture, rendering it incapable of undergoing the same action again.

Let us look a little closer at this action of small-pox poison. If the minutest particle of substance, a little dried albuminoid substance, in a peculiar chemical state of action, on a lancet, or in the dust of the air, is put into the cellular tissue or is inhaled into the lungs, it passes on to the blood, and through it into every texture. In a few days the chemical actions of oxidation and nutrition throughout the body are altered, and the particle of matter has reproduced itself immeasurably. The violently increased chemical action, the peroxidation, is shown by the increased heat of the body, the violent fever. The altered nutrition is evident not only in the eruption of pustules in the cellular tissue under the skin, but in the altered condition of the blood and in all the textures of the body; each particle of substance being rendered incapable of undergoing the same process again, and by assimilation every future particle that takes the place of every modified particle is also generally incapable of being modified again.

Throughout the course of the general peroxidation, and more especially at the end of the fermentation, local peroxidations frequently come on in any part of the body. Inflammations of the eyes, the ears, the mucous membranes, the joints, the serous membranes, the parenchymatous tissues, anywhere an unmodified peroxidation is ready to begin, and this easily gives rise to suppuration or causes obstructions which the feeble circulation cannot overcome.

The most striking facts concerning this small-pox ferment are, first, the very small quantity of substance that produces so much effect; secondly, the immeasurable increase of the poison in the body, each pustule having the same property as the original ferment; thirdly, the period of incubation during which the poison must at first slowly increase in every texture, and there give rise to the modified peroxidation and altered nutrition which constitute the attack.

The poison of scarlet fever, of measles, and of typhus, though less tangible, are not less substantial than the small-pox ferment. Like it, they can most probably be dried and carried from place to place and pass into the mouth with the dust which we each moment inhale or swallow. In chemical composition scarlet fever, measles, and typhus ferments most probably resemble albumen in complexity, and, like albumen, they may be altered in composition and action by heat, alcohol, arsenic, tar acids, and many metallic salts. As soon as they reach any spot where they can oxidise, they set up an oxidation and reproduction in each contiguous particle of albuminous substance. From the cellular tissue, the air passages, or the stomach or bowels, the contact action spreads into the blood, and there it multiplies, whilst it is carried into all the capillaries, and through them into every texture of the body; then the increased oxidation and formation of ferment becomes most violent, and fever to a greater or less degree is present. Long after the strongest action is reached, a slower action continues, and at any time or in any part or texture of the body, whilst the specific chemistry is going on, an ordinary local peroxidation may be lit up, and a more or less acute inflammation may be added to or follow the fever which the ferment had produced.

During the height of the fermentation in typhus fever, the heat may rise to 5, 6, or even 10° Fah. above the ordinary temperature; and when the fermentation is ended, the albuminous textures of the body are so changed that they are incapable of going through the same process again. Between these two results there are innumerable other products of chemical change, varying with the kind and degree of fermentation. In typhus fever it is said that urea is increased and carbonic acid diminished. To these and a multitude of other chemical questions regarding fermentation chemistry will give definite answers; but above all questions, one of the most difficult to answer and yet one of the most important, is the amount of oxygen that is consumed in the different kinds and degrees of peroxidation which can take place within us.

In each organ, according to the intensity of the action set up by the ferment, altered functions may arise, and these may be still more altered when an ordinary peroxidation at the same time takes place. Thus the brain, heart, lungs, kidneys, liver, or any texture composing these organs may show by more or less wrong mechanical results the effect of the ordinary or modified peroxidation; and the effects of the fever and of the inflammation may be so mixed that neither during life nor after death may any accurate separation be possible.

Closely related in chemical composition to these violent ferments are the less active ferments of ague and typhoid fever. There is so little difference in the chemical composition of animal and vegetable substances that the distinction between animal and vegetable poisons is no longer possible. Vegetable albuminous matter undergoing change may produce almost, if not quite, exactly the same poison as animal albuminous matter. Hence, probably, the resemblance between ague poison and typhoid fever poison, and the possibility that sometimes one and sometimes the other of these poisons may be formed from the same changing matter under different circumstances.

Ague ferment is probably a highly complex nitrogenous substance, capable of being dried and carried by the wind far from the place where it was produced. It enters by the mouth with the dust, and, like animal or vegetable alkaloids, it passes from the blood into every texture of the body, and acts on each much or little according to its chemical properties. Probably it acts most strongly on the nerves that regulate oxidation, causing for a time contraction of the arterial vessels and consequent suboxidation everywhere. The increased obstruction of the small arteries reacts on the tension of the blood, and this produces increased contraction of the heart, which continues to increase until the obstruction yields and a state of peroxidation is set up by which the poison is partially destroyed. During the remission, probably the poison is reproduced until sufficient, in from one day to three days, is formed to go through the same action again.

This theory of ague admits of a reasonable explanation of the action of quinine and arsenic in stopping the paroxysms of the complaint. On the ague poison itself quinine and arsenic may have no action, but they pass into every texture from the blood, and, combining with the nervous substance on which the ague poison acts, they form a compound on which the ague poison is incapable of producing an effect before it is oxidised and destroyed.

The ague poison, unlike the small-pox or typhus fever ferment, instead of protecting the body by making it incapable of undergoing the same action again, makes the nerves more ready on the slightest renewal of the poison to undergo the same action again; so that it has been said that the ague poison may lie dormant for years. It is far more probable that a much smaller quantity of the poison can produce the return of the symptoms than that the original ferment should retain its properties for months, or even for years, after its first action had passed by. In this respect, and in some others, the action of ague poison proves that it is a very peculiar ferment, and hence, though I have placed it near to the typhoid ferment because of its origin, I must shortly point out to you the different effect which the true typhoid ferment produces.

The typhoid ferment is probably formed out of vegetable or animal albuminous substance. In sewers, in drains, in ditches, possibly even in the drains of the human body, a substance may be formed which is not volatile in itself, but by foul gases or currents of air can be carried into the mouth, and in some period between a few hours and fourteen days it sets up a modified peroxidation. More slowly absorbed and less rapidly reproduced and changed than typhus ferment, it passes into the blood, and from it into each texture; whilst some of the poison has a local action on the glands of the small intestine, and produces increased action, effusion, obstruction, and retrograde action, causing ulceration, sloughing, and even perforation, by which mechanically the contents of the bowel may escape and an uncontrollable simple peroxidation may be set up. The poison that has passed into the tissues acts on each organ more slowly than the typhus poison; still, like it everywhere, it gives rise to altered functions, and everywhere local peroxidations are ready to occur; bronchitis, pneumonia, peritonitis, tubular nephritis, cystitis—any of these and many other inflammations may be set up at any time during the course of the fever. Probably the substances produced by the increased chemical action in typhus and typhoid fever will be found to be very similar. There may be the same amount of heat, the same excess of urea, the same excess of antecedent substances from which the urea is formed; possibly the same consumption of oxygen when the same temperature in each fever occurs; but in the properties of the ferment formed in the body a distinct difference of diffusibility must exist, the typhus poison passing with greater ease into neighbouring bodies; whilst the typhoid poison rarely, if ever, is communicated by infection.

On the Mechanical Disorders that arise out of the Chemical Errors in Fevers.—In all fevers the loss of mechanical power is quite as remarkable as the increase of chemical action. The chief amount of energy liberated by the action of oxygen in the body seems expended in the production of heat, so that far less than the ordinary amount of power remains to be employed in the production of mechanical motion. The muscles may be considered as machines made for the conversion of chemical force into mechanical motion. How this is done cannot be explained in the present state of our knowledge of the mechanical, chemical, or electrical actions in the muscle; but that the muscular force arises from some equivalent force, and sooner or later must come from the chemical force in oxygen, hydrogen, and carbon, opens an immense field for investigation, and is easier of belief than that force should be each moment created and destroyed. The amount of sugar and fat present in any muscle would soon be used up if in the working of the muscle itself fresh fuel was not produced. The action of oxygen on the syntonin in the muscle may be direct, and may give rise to the force required; but it is more probable that the syntonin splits into substances of two classes, one ending in urea, which is incapable almost of combustion, the other in inosit, which would immediately give water and carbonic acid.

In fever the poisonous ferment in the muscle probably determines a different chemical action from that which takes place in the muscle in health. The increased heat and increased urea mark the increased action, but loss of motor-power in the muscles shows that the conversion of chemical into mechanical force does not take place.

This mechanical disorder becomes by its action on the muscles of respiration or circulation the source of complications and dangers in fever to which I must shortly allude. Gradually in the course of fevers the sounds of the heart may be found to become more and more feeble, and the respiration without any wrong in the lung, becomes shallow and weak from the diminished power in the muscular tissue. The diminished tension in the arteries has a direct effect upon the circulation through the capillaries, and the motion in the veins is more or less stopped; hence congestion of blood in the venous system occurs, hemorrhages, effusions, and coagulations in the veins may take place anywhere. The imperfect action of the muscles of respiration produces the same mechanical effects in the circulation through the lungs; imperfect oxygenation takes place in the lungs from the stoppage of blood in the pulmonary veins; without any inflammation, œdema of the lungs, hypostatic consolidation may occur. The circulation through the lung is so feeble that even the force of gravity acting on the blood in the lungs cannot be counteracted, and accumulation takes place in the most dependent parts.

The muscles of the bladder are also so weakened that the urine accumulates, and frequently external muscular pressure is required after the catheter has been passed to cause the urine to flow.

In another large class of zymotic diseases the qualitative and quantitative errors of oxidation are scarcely detectable, whilst the qualitative and quantitative errors of nutrition chiefly mark the action of the poison.

Of these diseases true syphilis may be taken as the type.

It can scarcely now be doubted that the actions of two poisons have been included under the term syphilis. The first, like impetigo, is capable of being communicated, and often repeated because it exists only locally or passes up to the nearest lymphatic glands; whilst in the true syphilis the poison from the local sore enters the blood and pass from it into each texture, where it multiplies and produces changes of nutrition, and partly unchanged, and partly changed in composition passes out perhaps in each secretion.

This true syphilitic ferment resembles very closely the small-pox ferment in the universal diffusion of the poison, and in the consequent protection it gives from another attack by rendering a second similar change in each texture impossible. The protective power of the alteration is to a slighter degree extended to the progeny through the germ and spermatozoon; so that a race partly protected by inheritance may suffer less from these diseases than a purer race, whose textures are free to undergo the full change which constitutes the disease. Both persons give rise to increased cell growths, effusions, oxidations, congestions, and ulcerations; and these may take place in any part of the body, for the poison exists everywhere.

True syphilis differs, however, altogether from small-pox in its definiteness of course as to time. Syphilis produces no fever to terminate the fermentation in a definite period, and it may consequently remain active or dormant in the textures for years.

It is vain now to ask what circumstances at the end of the fifteenth century produced the first modified albuminoid matter which gave rise to the first true syphilitic poison. In cancer, which bears a distant resemblance to syphilis, although the spontaneous generation of the first cancer cell is daily occurring in some predisposed texture, yet we are as yet quite unable to say what produces the first modified particle of matter which multiplies and communicates its composition to adjacent predisposed textures by contact, and is carried by lymphatics and blood vessels to every part of the body, and affects the nutrition of each part with which it comes in contact, provided the textures are in a condition to propagate the cancer cells.

Another instance of spontaneous generation of a poisonous ferment is presented to us in rabies; and with this poison also unless a peculiar condition of system exists the ferment when inserted has no action; and here also our knowledge is at present unable to say what circumstances determine the formation of the first particle of poisonous saliva; except by its effects, the peculiar change in the albuminoid matter of the saliva in the present state of chemical knowledge could not be recognised. —*Med. Times and Gazette, March* 17, 1866, *p.* 275.

2.—ON THE THERMOMETER IN SPECIFIC FEVER.

The subject of the temperature of the body in disease is exciting much attention at the present time in the metropolitan hospitals. A few words respecting the thermometers in use will probably be interesting to such of our readers as may wish to commence observations for themselves.

The instruments which we have seen employed are of English manufacture. A cheaper form, of German make, may be procured; but it is said that these are not so exact or convenient. The thermometer is either simple or self-registering. The former must be read *in situ;* the latter can be removed from the patient, and read when convenient. It may be applied, therefore, by the nurse during the absence of the medical attendant, who can examine its record at his next visit. The contrivance by which the instrument is self-registering is very ingenious. It was suggested more than twenty years ago by Professor John Phillips, of Oxford, that, as no thermometer was perfectly free from air, the portion of air present might be utilized. By a little manœuvring a small portion of the column of mercury was detached from the remainder, the speck of air occupying the intermediate space. It is evident that, as the column rises under the influence of heat, this detached portion, or index, is carried before it. When the column recedes under contraction, the index remains at the point to which it had been propelled. This is a great improvement on Rutherford's register, in which the index was of steel, and could not be relied upon.

Dr. Aitken, to whom is largely due the credit of introducing this interesting subject into the medical practice of England, has arranged, with the aid of Mr. Casella, the Admiralty instrument maker, two thermometers, graduated in Fahrenheit degrees, for the especial use of medical practitioners. One is an ordinary, but very sensitive, instrument, made with a curve, so that the bulb may be easily fitted into the axilla. The other is straight, and on the self-registering principle. Dr. Ringer employs a slighter and more sensitive instrument, without a register. Dr. Gull has had the registering thermometer made a few inches shorter, and thereby rendered it much more portable. This latter instrument answers perfectly, but requires a little care in its use. The column of mercury, being necessarily shorter than in the ordinary thermometer, sinks, at a low temperature, into the bulb ; and if the instrument be shaken under these circumstances (and a kind of jerk is necessary to arrange the index), there is danger of the speck of air getting displaced, and the self-registering principle consequently destroyed. This mischance, however, is easily obviated by warming the bulb before the necessary jerking movement is made.*

During all the acute specific fevers, the temperature of the body is abnormally elevated ; and, as is the case with acute inflammation of any of the tissues of the body, the severity and duration of the fever are exactly measured by the degree and duration of the elevation of the temperature. This elevation of the temperature is constant. This, however, is not the case with the other symptoms met with in typhoid fever ; for the pulse may remain normal throughout the attack, and the tongue may be clean ; whilst even, in rare cases, during the greater part of the attack, the appetite may not be greatly impaired. It is thus evident that the temperature affords more reliable information respecting the severity of the attack of typhoid fever than the other symptoms. The invasion of typhoid fever is almost invariably gradual. This is well shown by the temperature. During the first four days of the attack the elevation of the temperature gradually increases ; but, at the end of the first week, it has usually nearly attained to the maximum temperature of the attack. It is true that this gradual increase of the disease is also shown by the other symptoms, and especially by the gradual increase of the weakness of the patient. The temperature of the body, however, does this more precisely. The other acute specific fevers, and all inflammations, mostly begin abruptly. The other disease that corresponds to typhoid fever in respect of the gradual invasion of the disease, is tuberculosis. Thus if we meet with a malady the invasion of which has been gradual, we mostly have to do either with typhoid fever or tuberculosis. Though at present the temperature of the body has not afforded us much information in respect of the treatment of typhoid fever, it assists us greatly in arriving at a correct diagnosis and prognosis.

First, in respect of the diagnosis. During the first three or four days, beyond the gradual increase in the elevation the temperature is in no way characteristic ; but if on the fourth or fifth day the maximum temperature attained during the twenty-four hours be not 103·5°, the disease is most probably not typhoid fever. And further, if on the first or second day the maximum temperature reaches 104° the disease is some other than typhoid fever. The information given us by the temperature is great, for often the diagnosis of typhoid fever at its commencement is difficult, inasmuch as the characteristic rash does not usually appear before the sixth, and may be postponed to the twelfth, day of the disease ; and, indeed, in children, is not unfrequently absent throughout the attack. The temperature affords still greater information in respect of the prognosis. It is during the second week that such information is obtained. From the behaviour of the temperature during this time we can speak confidently as to whether we have a mild or severe case to treat.

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* All these thermometers can be obtained of Mr. Casella, 23, Hatton-garden. The case of Dr. Aitken's thermometers contains some very useful remarks upon the employment of the instrnment, extracted from Aitken's "Science and Practice of Medicine."

In typhoid fever we have always at one period, and sometimes throughout the attack, great daily variations in the temperature. When such occurs, it is usually low in the morning, and attains its maximum in the evening, mostly between seven and twelve p.m. The greater these fluctuations at the end of the second week the more favourable is the attack and the shorter will be its duration. If the temperature falls considerably in the morning—that is to say, if it sinks below 102° Fahr., —even though the evening rise may be great, the prognosis is favourable. The greater the fall the more favourable is the prognosis. On each succeeding day the amount of the morning fall increases. If during this period (second week) the maximum temperature be great, the disease is proportionately severe ; thus a daily rise to 105° indicates a severe, and to 106° a still more severe attack. Should the temperature during the second week of the fever remain persistently high, we may expect a severe and long-continued attack. Thus a continuous elevation of the temperature at 104° Fahr. is serious, whilst a morning temperature of 104° with an evening rise to 105° and 106° is still more serious, and calls for a grave prognosis. Such cases are, however, by no means always fatal.

In these cases of persistent elevation of the temperature, the earliest sign of improvement is a fall of the morning temperature. When such a fall occurs, and especially if it be repeated on the subsequent days, even though the maximum temperature reached in the evening remain the same, we may be sure that the fever has begun to decline. Though the temperature thus affords early and great information respecting the degree of severity of the attack, still it is not the only condition that determines the prognosis. It is necessary to take into consideration the state of health of the patient previous to the attack. If this has been good, the prognosis is far more favourable than if the health has been impaired by excesses of any kind or by bad hygienic conditions of life. The problem to be solved is—Given a disease of known severity (and the temperature affords us this information in typhoid fever), can the life of the patient be supported during this period ?

The temperature affords us still further information. Thus in typhoid fever, as in all febrile cases, a sudden increase in the elevation of the temperature above what has occurred on previous days proves the existence of some intercurrent disease. The temperature often gives us this information when the intercurrent disease would pass undiscovered ; for the symptoms in such cases are often either masked by those of the first disease or are entirely absent. Thus if in typhoid fever the temperature should one day rise to a much greater height than on the previous days, we must not rest satisfied until the cause of this second elevation has been ascertained. Such intercurrent disease adds to the gravity of the prognosis.

A sudden fall in the temperature in typhoid fever may be due either to diarrhœa or hemorrhage into the intestine. If it occurs very suddenly, it is most probably due to the latter. Usually, however, other and equally characteristic symptoms arise to indicate such an occurrence.

Diarrhœa, if often repeated, whether it lower the temperature or not, must be checked, as diarrhœa causes great prostration even in health, and consequently produces still greater weakness, sometimes sufficient to cause death, in persons suffering from typhoid fever. It is best controlled by injections of starch of the consistence of cream, and varying from one to two ounces in quantity ; to this some laudanum, or one of the metallic astringents, especially the former, may be added with benefit.

After the temperature has fallen greatly or become normal, should it again begin to rise gradually, we are justified in diagnosing either a relapse or the commencement of an attack of tuberculosis—most probably the former ; for relapses more commonly occur after typhoid than any other of the specific fevers. Such relapses are especially apt to occur in certain epidemics. If the temperature indicates a very mild attack, we may expect the disease to terminate between the fourteenth and twentieth days. Should it indicate a more severe attack, the disease will probably last from twenty-five to thirty days ; in very severe and very rare cases it is said to last even six or eight weeks.—*Lancet, Dec.* 9, 1865, *p.* 647.

3.—THERMOMETRICAL OBSERVATIONS ON THE FEVERS OF CHILDREN.

By G. Stevenson Smith, Esq., Resident Medical Officer, Royal Edinburgh Hospital for Sick Children.

[More than a century has elapsed since the value of the thermometer in the diagnosis of disease was first pointed out, but it has not received so much attention as its importance demands. This has probably arisen from the facility with which increase of temperature of the body or a part may be judged of by the hand of the physician—the thermometer however corrects and renders exact this more ready method. It will probably prove more useful in fevers and acute inflammations, and in cases of suspected phthisis.]

A knowledge of the state of the temperature is of the utmost consequence in enabling the physician to arrive at a correct diagnosis in many of the affections of children; and it is here, I think, more than in the case of adults, that the medical man will derive the greatest benefit from the use of the thermometer. For how puzzling it often is, in the first stage of a febrile affection in a child, to make out with certainty the real nature of the disease. And how frequently does an illness, which was ushered in by almost all the symptoms of fever,—a quick pulse, headache, thirst, loaded tongue, &c.,—deceive both the practitioner and the parents, and pass away almost as rapidly as it came! Every one knows the difficulty of making a correct diagnosis in many of the diseases of childhood; and as an evidence of this, patients are frequently sent into the fever wards of an hospital, whose illness turns out to be only trifling, and never goes on to fever at all. Now, it is in such cases that the temperature of the body, as ascertained by the thermometer, affords information which renders mistake impossible.

Before proceeding to report the cases individually, it may be of some interest to make a few remarks regarding the instrument used and the mode of applying it.

The Instrument.—The instrument used in making my observations was Aitken's curved thermometer, made by Casella of London; and while I have every reason to be satisfied, both with its delicacy and its accuracy, I beg to offer a few suggestions in regard to its construction, which, if carried out, would, in my opinion, be very useful. I have found in using it, especially on a dark day, or in gas-light, that it is extremely difficult to make out with exactness the figures and markings on the glass. It would be an immense improvement were the numbers and degrees tinted with black, or any other colour, so that the eye could more readily distinguish the height of the mercurial column. It would also be advantageous, in making observations on young children, to have the curved portion of the instrument made considerably shorter.

How to apply the Thermometer.—In order to obtain the true state of the temperature of the body, the thermometer must be applied to some part which is not exposed to the air ; and the best and most convenient place to insert the instrument is in the axilla. With a child in bed this is easily and accurately done, by making the patient lie on the back, and then introducing the bulb of the instrument under the anterior margin of the pectoral muscles, so that the mercury is surrounded by the soft parts. A better way than this, however, is to make the child lie on the side, and introduce the bulb into the arm-pit from behind. I have found that in this way the instrument is more firmly and closely embedded in the parts, and the stem passing up behind the shoulder is not breathed upon.

The thermometer should be allowed to remain in this position as long as the mercury continues to rise. From three to five minutes is the time usually required. It saves time to heat the bulb a little in the hand before introducing it.

The height of the mercury must be noted while the instrument is *in situ.*

The Normal Standard of the Temperature in Children as compared with that of Adults.—It has been ascertained, by numerous and carefully-conducted experiments made by Davy and others that in adults the normal temperature of the body is about 98·4° Fahr. In infants, according to the observations of M. Roger and Dr. Holland, the temperature is somewhat higher, when they are placed in conditions favourable to its sustenance. When examined immediately after birth, it is nearly 100°, but it quickly falls to 95·5°, and again rises in the course of twenty-four hours to 97·7° in weakly subjects, and to 99·5° in strong ones. Between four months and six years of age M. Roger found the average temperature to be 98·9°, and between six and fourteen years 99·16° (Carpenter's Physiology). Dr. Bennett, again, in his Outlines of Physiology, states that in children the heat of the body is about 2° higher than in adults. As the ages of the patients on whom the following observations were made range from three to ten years, I think we shall be pretty nearly correct if we assume the normal standard to be 99°. In disease the temperature has been known to rise as high as 112°, and to sink as low as 67°.

Three Cases illustrative of the Value of the Thermometer as a Diagnostic Agent.—*Case* 1.—M. M., aged 8, was admitted on 5th January 1866. She was said to be suffering from fever, her illness having commenced five days previously with rigors and headache. On admission, the tongue was slightly coated, and red at the tip; pulse quick, but there was no rash. The thermometer soon removed all doubt, and enabled us to say with certainty that the child had no fever, and she was consequently sent home.

M. indicates morning, E. evening.

			Pulse.	Resp.	Temp.				Pulse.	Resp.	Temp.
Jan.	5.	E.	108	48	99°	Jan.	7.	M.	84	30	95°
	6.	M.	108	30	98			E.	84	30	95 2-5ths
		E.	96	30	98						

The gradual fall in the temperature to 95° is remarkable.

Case 2.—R. N., aged $3\frac{1}{2}$, was admitted on January 13, 1866, said to be affected with fever; his illness having been preceded about a week previously by cold shivers, headache, and thirst. When admitted he had a foul tongue, quick pulse, and other symptoms of continued fever; but the temperature never rose above $98\frac{1}{5}$°, and soon went down to $95\frac{3}{5}$°, so that it became evident that he had no fever, and he was accordingly dismissed.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Jan. 14.	M.	130	32	98 1-5th°	Jan. 15.	M.	110	32	97°
	E.	96	32	95 3-5ths	16.	M.	100	32	96 1-5th

Case 3.—C. F., aged 6, was admitted on January 2, 1866. Ill about a week, the illness having commenced with vomiting, headache, &c., and it was thought that she had contracted fever. On admission the pulse was quick, tongue foul, but, as will be observed, the temperature never rose above the normal standard, and she was dismissed quite well.

		Pulse.	Resp.	Temp.
Jan. 2.	E.	120	36	98 2-5ths°
3.	M.	96	36	96
	E.	96	36	95 2-5ths

Here again the unusually low temperature of $95\frac{2}{5}$° was observed.

Cases of undoubted Typhus, with daily Records of the Temperature, the Respirations, and the Pulse.—In all of the following cases the eruption of typhus was distinct. Most of them were admitted about the end of the first or the beginning of the second week, but it is always a difficult matter to make out with absolute certainty the very day on which a fever begins. We usually count from the day the child took to bed, or from the day it last appeared to be in its usual health.

Case 1.—J. J., aged 8, was first admitted, said to be ill with fever, on December 13, 1865, but he never showed any symptoms of fever, and was sent out apparently quite well in a day or two. Very soon afterwards, on December 26, he was readmitted on the recommendation of a medical man, who thought the boy was suffering from typhus. He had a fresh colour, a moist clean tongue, and appeared to be quite well, so that the nurse imagined that the boy was scheming. The steady rise of the temperature, however, indicated that some fever was impending, and the appearance on the 29th of the typhus rash confirmed the accuracy of the thermometer.

		Pulse.	Resp.	Temp.
Dec. 27.	E.	108	24	99°
28.	M.	108	24	101 2-5ths
	E.	120	32	103 3-5ths
29.	M.	108	36	102 4-5ths
	E.	120	32	103 3-5ths
30.	M.	120	36	102 4-5ths
	E.	128	37	104 1-5th
31.	M.	136	40	104 1-5th
	E.	140	40	103
1866.				
Jan. 1.	M.	132	40	101 3-5ths
	E.	132	36	102 2-5ths
Jan. 2.	M.	136	38	102°
	E.	152	36	102 3-5ths
3.	M.	120	36	100
	E.	115	36	102
4.	M.	120	36	101 1-5th
	E.	108	36	100
5.	M.	108	36	99
	E.	96	36	99 3 5ths
6.	M.	84	32	95 3-5ths
7.	M.	84	32	95
16.	M.	84	32	97 4-5ths
24.	M.	84	30	97

In this case the height of the fever was reached on December 31st, when the thermometer stood at $104\frac{1}{5}$°; after this defervescence went on gradually, and the fall to the normal standard on January 5th showed that the crisis had arrived. He improved after this date, and was dismissed quite recovered on January 29, 1866.

Case 2.—E. C., aged 9, admitted December 21, 1865. Ill about seven days; typhus rash distinctly visible. The attack was a severe one: there was a good deal of raving for some days, and the boy was deaf and stupid.

		Pulse.	Resp.	Temp.
Dec. 21.	E.	120	28	103 1·5th°
22.	M.	132	34	102 2-5ths
	E.	132	32	103
23.	M.	132	32	102 4-5ths
	E.	132	36	103 2-5ths
24.	M.	130	32	103 2·5ths
	E.	132	32	103
25.	M.	130	32	102
	E.	144	36	103 1-5th
26.	M.	120	34	102 3-5ths
	E.	130	34	101 2-5ths
27.	M.	120	28	103
	E.	108	36	101 2-5ths
Dec. 28.	M.	108	32	101
	E.	114	28	100 1-5th°
29.	M.	96	30	99 3-5ths
	E.	96	24	99 4-5ths
30.	M.	96	24	98 2-5ths
	E.	100	24	99
31.	M.	100	24	97 3-5ths
	E.	96	20	98
1866.				
Jan. 1.	M.	80	20	96 4-5ths
2.	E.	72	20	96 3·5ths
4.	M.	72	24	97 3-5ths
11.	M.	72	24	97 2-5ths
24.	M.	72	24	97

This patient made an excellent recovery, and was dismissed on January 29, 1866.

Case 3.—W. M., aged 10, admitted 17th January, 1866. Ill five days; typhus rash well marked.

		Pulse.	Resp.	Temp.
Jan. 17.	E.	132	24	103 2-5ths°
18.	M.	120	36	103
	E.	120	24	104
19.	M.	120	36	103
	E.	132	36	103
20.	M.	124	36	102
	E.	124	36	102 4-5ths
21.	M.	120	40	102 2-5ths
	E.	120	34	102 4-5ths
22.	M.	120	36	101 2-5ths
	E.	120	36	103 1-5th
23.	M.	132	36	102
	E.	132	36	102 4-5ths
24.	M.	124	36	102
	E.	120	36	103 3-5ths
Jan. 25.	M.	120	36	100 3-5ths°
	E.	124	36	103
26.	M.	120	30	101 3-5ths
	E.	108	30	101
27.	M.	96	30	99 3-5ths
	E.	108	30	100 3-5ths
28.	M.	100	30	98 4-5ths
	E.	90	30	99 1-5th
29.	M.	84	30	96
	E.	84	30	97 3-5ths
30.	M.	84	24	95 3-5ths
	E.	60	24	97
31.	M.	72	24	97 3-5ths
Feb. 2.	M.	84	24	97 1-5th

In this case there was epistaxis, pain, and gurgling in the belly, and almost constant noisy delirium for three days. On the 28th January, the temperature fell to 98⅘°, and he became much calmer and better. On the 29th, he vomited everything he swallowed, and seemed much worse, but as there was no rise in the temperature we were assured that nothing serious was going to happen. He made a good recovery.

Case 4.—J. M., aged 6, sister of the above, admitted 20th December, 1865; ill about six days; copious typhus rash on face, hands, and legs.

		Pulse.	Resp.	Temp.
Dec. 20.	E.	136	28	102 1-5th°
21.	M.	144	30	101 3-5ths
	E.	132	33	103
22.	M.	144	32	101
	E.	144	30	100 4-5ths
23.	M.	120	32	101 1-5th
	E.	140	36	103 4-5ths
24.	M.	126	30	101 3-5ths
	E.	140	34	103 3-5ths
25.	M.	120	40	100 4-5ths
	E.	120	36	101
Dec. 26.	M.	120	30	99 2-5ths°
	E.	120	30	100 2-5ths
27.	M.	96	36	98
	E.	96	32	92
28.	M.	100	28	97
	E.	96	28	98 4-5ths
29.	M.	84	20	97 2-5ths
	E.	84	24	97
31.	M.	96	24	6 4-5ths
1866.				
Jan. 1.	M.	96	24	96 3-5ths
4.	M.	96	24	96 4-5ths

It will be seen that in this case the temperature fell on the 14th day of the fever, viz. the 27th December, to 98°. On the same day patient had a long sleep, and when she awoke she was much better. She improved daily, and was dismissed recovered on 20th January 1866.

Case 5.—J. M., aged 4, admitted 15th January 1866. Ill five days. Typhus rash came out on 17th January.

		Pulse.	Resp.	Temp.
Jan. 15.	E.	132	46	103 3-5ths°
16.	M.	144	42	103 4-5ths
	E.	138	36	102 1-5th
17.	M.	144	42	102 3-5ths
	E.	156	46	103
18.	M.	156	48	101
	E.	144	48	102 2-5ths
19.	M.	144	48	101 2-5ths
	E.	140	48	101 3-5ths
20.	M.	144	48	101 4-5ths
	E.	144	48	101 3-5ths
21.	M.	132	36	102 2-5ths
	E.	132	36	106 1-5th
Jan. 22.	M.	108	42	98 1·5th°
	E.	108	36	99 4-5ths
23.	M.	120	36	99 4-5ths
	E.	120	36	100 3-5ths
24.	M.	120	30	99
	E.	108	36	100 2-5ths
25.	M.	120	36	99 3-5ths
	E.	112	36	100
26.	E.	96	36	98 3-5ths
27.	E.	96	36	97 2-5ths
28.	E.	96	36	97 1-5th
30.	E.	80	24	95

Here the temperature fell to $98\frac{1}{5}$° on the 22nd, but it afterwards rose, on several occasions, to 100°; indicating that the course of the convalescence was irregular. The child, however, made a good recovery.

Case 6.—J. B., aged 5, admitted 5th January 1866. Ill about seven days. Typhus rash on face and legs.

		Pulse.	Resp.	Temp.
Jan. 5.	E.	144	36	101°
6.	M.	144	48	101
	E.	156	34	102 2-5ths
7.	M.	132	36	100
	E.	144	42	102
8.	M.	132	42	101
	E.	152	40	103 4-5ths
9.	M.	152	32	102
	E.	152	36	102 3-5ths
10.	M.	144	48	101 4-5ths
	E.	132	36	102
Jan. 11.	M.	144	36	100°
	E.	144	36	102
12.	M.	132	36	100
	E.	144	36	99 1-5th
13.	M.	132	36	98 4-5ths
14.	M.	124	28	97 3-5ths
	E.	120	28	97 4-5ths
15.	M.	120	28	95 2·5ths
16.	E.	108	24	98 1-5th
21.	E.	96	36	95 3·5ths
Feb. 2.	E.	84	24	95 3-5ths

On the evening of the 12th January, it will be seen that, although the pulse numbered 144 beats in the minute, the temperature was down to near the normal standard $99\frac{1}{5}$°. This indicated the turn, for the boy did well ever after, and was dismissed recovered on 5th February. It will be observed that even at night, on two occasions before he left the hospital, the temperature remained so low as $95\frac{3}{5}$°.

Case 7.—E. M., aged 7, admitted 20th December 1865. Ill eight days. Typhus rash distinct on hands, forearms, and thighs.

		Pulse.	Resp.	Temp.
Dec. 20.	E.	152	42	104°
21.	M.	156	41	102 3-5ths
	E.	156	42	103 3-5ths
22.	M.	132	42	101 2-5ths
	E.	144	42	103 1-5th
23.	M.	144	44	102
	E.	156	52	102 4-5ths
24.	M.	144	38	102 2-5ths
	E.	132	42	102 2-5ths
25.	M.	108	40	101
	E.	108	37	100
Dec. 26.	M.	108	40	99°
	E.	108	40	100
27.	M.	108	34	99 2-5ths
	E.	108	36	99 4-5ths
28.	M.	108	34	97 1-5th
	E.	108	32	97 3-5ths
31.	M.	92	32	97 2-5ths
1866.				
Jan. 2.	M.	84	24	98 1-5th
9.	M.	96	24	95 1-5th

Here, again, the temperature which up to the 26th December, the fourteenth day of the fever, had always been above 100°, fell on that day to 99°, and though the pulse kept up for a day or two longer, still the boy improved from that date. He left the hospital quite well on 15th January 1866.

Case 8.—E. C., aged $8\frac{1}{2}$, admitted 21st December, 1865. Ill about eight days. Copious typhus eruption on face, hands, trunk, and legs.

		Pulse.	Resp.	Temp.
Dec. 21.	E.	132	28	103 3-5ths°
22.	M.	132	28	101 4-5ths
	E.	144	28	103 3-5ths
23.	M.	144	30	101
	E.	144	30	102 4-5ths
24.	M.	132	28	101 2-5ths
	E.	132	30	102 4-5ths
25.	M.	120	24	98 3-5ths
	E.	120	36	101 4-5ths
26.	M.	96	36	96 4-5ths
	E.	96	36	98
Dec. 27.	M.	96	34	97 2-5ths°
	E.	84	36	96 2-5ths
28.	M.	84	36	96 4-5ths
	E.	84	32	96 2-5ths
30.	M.	84	30	95 4-5ths
1866.				
Jan. 2.	M.	72	28	96 3-5ths
3.	M.	72	28	95
6.	M.	72	28	97

On the 26th of December, the fourteenth day of the fever, the temperature fell suddenly from $101\frac{4}{5}$°, at which it stood on the previous evening to $96\frac{4}{5}$°, no less than five degrees; thus indicating that the crisis had arrived. After this date the child improved every day, and went home quite well on 11th January 1866.

Case 9.— M. B., aged 8, admitted 26th December 1865. Had been ill about eight days. This was also a case of true typhus.

		Pulse.	Resp.	Temp.
Dec. 26.	E.	120	42	103 3-5ths°
27.	M.	120	42	101 2-5ths
	E.	108	30	103
28.	M.	108	40	101
	E.	100	24	102
Dec. 29.	M.	108	36	100°
	E.	96	36	100
30.	M.	96	30	97 3-5ths
	E.	110	28	98 2-5ths
31.	M.	104	24	97 4-5ths
	E.	104	28	98

		Pulse.	Resp.	Temp.
1866.				
Jan. 3.	M.	88	24	95 3-5ths°
6.	M.	84	24	97 2-5ths
Jan. 16.	M.	84	24	96°
24.	M.	84	24	98

It is rather strange that in this case the fall of the pulse preceded that of the temperature; for while on the evening of the 29th December the pulse was 96°, the heat was 100°. The boy did well and was dismissed quite recovered on 29th January 1866.

Case 10.—E. M., aged 4, admitted 25th January 1866. Ill about a week. Typhus rash on face, hands, and body. Patient was very ill, restless, delirious, and very deaf for some days.

		Pulse.	Resp.	Temp.
Jan. 25.	E.	144	40	194°
26.	M.	132	40	102 2-5ths
	E.	144	40	102 2-5ths
27.	M.	144	42	102 2-5ths
	E.	142	40	102 2-5ths
28.	M.	144	44	102 1-5th
	E.	150	44	102 3-5ths
29.	M.	132	44	101 3-5ths
	E.	136	40	102 2-5ths
Jan. 30.	M.	132	44	100 2 5ths°
	E.	132	44	100 4-5ths
31.	M.	108	40	100
	E.	132	40	101 1-5th
Feb. 1.	M.	108	40	99 1-5th
	E.	96	38	98 4-5ths
3.	M.	100	36	97 3-5ths
4.	M.	96	36	97

This girl improved immediately after the temperature went down to 99$\frac{1}{5}$°, and made an excellent recovery.

These are all the cases of typhus fever on which thermometric observations have yet been concluded, and they may serve to show the general range of temperature of that disease in young persons.

Of late, typhoid fever has not been very prevalent amongst children: at all events, comparatively few cases have been admitted to this hospital. I give the temperature, etc., of one case which proved fatal about the twelfth day of the fever.

Temperature, Respiration, and Pulse, in a Fatal Case of Typhoid Fever.—M. S., aged 4; admitted 16th January 1866. Ill six days.

		Pulse.	Resp.	Temp.
Jan. 16.	E.	132	36	104 2-5ths°
17.	M.	132	36	103 1-5th
	E.	120	36	104
18.	M.	132	40	102
	E.	132	40	103
Jan. 19.	M.	138	46	103 2-5ths°
	E.	144	48	103 4-5ths
20.	M.	144	40	101 1-5th
	E.	144	46	103 2-5ths
21.	M.	168	46	104 1-5th
	5 P.M.	168	46	104 2-5ths

Child died at 7 P.M. of the 21st.

It is of interest to observe that before death there was a considerable rise in the temperature. It would have been very difficult to have diagnosed this to be typhoid fever by the eruption alone, for it was very scanty, and by no means characteristic. But there were the pea-soup evacuations, and a tympanitic distension of the belly; and the post-mortem examination revealed changes in the mesenteric and Peyerian glands. There was likewise effusion into the chest.

The next case shows the range of temperature in a scarlet fever patient, in whom the eruption was copious, and the tonsils ulcerated. The rash first appeared on the 22nd.

Case of Scarlatina, with Daily Records of the Temperature, Pulse, and Respiration.

		Pulse.	Resp.	Temp.
Jan. 22.	E.	150	36	104°
23.	M.	144	36	103
	E.	144	36	103 3-5ths
24.	M.	144	36	102 2-5ths
	E.	144	40	103 1-5th
25.	M.	120	36	100
	E.	140	36	102
26.	M.	120	36	99
	E.	120	30	99 1-5th
27.	M.	96	30	97 3-5ths
	E.	108	30	97 3-5ths
Jan. 28.	M.	92	30	97°
	E.	108	30	98 2-5ths
29.	M.	96	30	97 4-5ths
	E.	90	30	98 2-5ths
30.	M.	96	36	97
	E.	108	36	98 4-5ths
31.	M.	96	24	98 1-5th
	E.	90	24	98
Feb. 1.	M.	96	24	98 2-5ths
3.	M.	84	24	98 4-5ths

Desquamation began on the seventh day from the appearance of the eruption. There was no renal complication, and patient made a good recovery.

Of course, these cases, the particulars of which I have here recorded, are far too few to enable me to make any correct generalization as to the range of the temperature in the fevers alluded to. Nor do I intend to attempt to draw partial or premature conclusions; but convinced that "the foundation of all knowledge must be a careful and extensive acquisition of facts," I offer the results of my observations as a contribution to the thermometry of disease in children.

It is of great interest to remark that in *all the ten typhus cases the temperature fell considerably below the normal standard during convalescence:* and in seven of them it was so low as to be under 96° Fahr.

The greatest heat observed was in Case 1, where on two occasions the thermometer stood at $104\frac{1}{5}$°.

In the typhoid case the greatest heat was 104 2-5ths°, while in the scarlatina patient the maximum was 104°.

The lowest temperature occurred in the typhus cases 1, 5, and 8, during convalescence, when the heat of the body was only 95°.

There is just one other point I would beg to direct attention to. as it is of importance in connexion with the diagnosis of typhus from typhoid fever. In Dr. Aitken's recent work he thus alludes to the different modes of defervescence in the two diseases: "The difference between the two (typhus and typhoid) is rendered still more striking by their respective modes of defervescence. In typhus the defervescence shows no gradual remissions, on the contrary it is invariably sudden." And in his typical ranges of temperature, he represents the fall in typhus to be as much as five degrees, when the crisis occurs.

Now, in only one of my cases (No. 8) was the defervescence either so sudden or so great as this. In it the temperature, which, on the evening of December 25, was 101 4-5ths, fell on the following morning to 96 4-5ths,—a fall of five degrees. But in most of the other cases the decline of the temperature to the normal standard or under it usually took place gradually, the different steps downwards scarcely ever measuring more than one or two degrees.—*Edinburgh Medical Journal, March* 1866, *p.* 823.

4.—ON THE USE OF THE THERMOMETER AS A GUIDE IN THE DIAGNOSIS OF PYREXIAL DISEASES.

By Dr. F. W. Gibson, B.A., late House-Surgeon of the Taunton and Somerset Hospital.

Fluctuations of Temperature within the Limits of Health.—The normal temperature in the axilla, the only locality in which the temperature is now habitually taken, is 98·5 deg.; but in certain circumstances it may vary a degree or two above or below this point. These variations are, however, not persistent. In a few hours—at most, twenty-four—the temperature becomes normal. The important deduction from this fact is, that a *persistent* rise above 99·5 deg., or depression below 97·3 deg., is a sure sign of disease.

Exposure of the body to the air, even in warm weather, lowers the temperature considerably. I have frequently observed a depression of the temperature, continuing for some hours, after slight operations in which there was no notable shock or hemorrhage; *e.g.*, a boy, aged 11, admitted for phimosis and gonorrhœa, had a temperature of 100·4 deg. immediately *before* the performance of the operation of circumcision. For two hours *after* the operation, the temperature remained below the normal height, although he was in bed, carefully covered up.

The temperature is lowered after a full meal and the use of alcohol. It rises as digestion advances. Ingestion of improper articles of diet in typhoid (and other fevers?) may cause a remarkable rise in the temperature, as is shewn by the following case. A man, aged 21, in the third week of typhoid fever, becoming convalescent, with a temperature nearly normal, ate, at two in the afternoon, a considerable quantity of heavy currant-cake. At eight in the evening, his temperature had risen to 105·6 deg.

The temperature is lowered by sleep.

Correlation of the Pulse and Temperature. The rule, "that an increase of temperature of one degree above 98° corresponds with an increase of ten beats of the pulse per minute," though true in the main, is, as shown by the following cases, subject to many exceptions. In typhoid fever, I have known a pulse of 100 to exist with a temperature of 105·6 deg.; in jaundice, a pulse of 58 with a temperature of 103; in rheumatic fever, a pulse of 80 with a temperature of 103·5 deg.; in peritonitis, a pulse of 80 with a temperature of 102·2 deg.; in tetanus, a pulse of 72 with a temperature of 104 deg.; in shock following severe surgical injury, a pulse of 120 with a temperature of 98·5 deg.; in typhoid fever, a pulse of 120 with a temperature of 100·4 deg.

The temperature is, therefore, a much more certain guide than the pulse, since it is not liable to be affected by nervous debility or excitement. The temperature is, in fact, the only certain means of diagnosis between the frequency of pulse produced by weakness, and that caused by fever; and in those cases, also, in which extreme slowness of the pulse exists as an idiosyncrasy, the employment of the thermometer leads to the discovery of disease which would not otherwise be detected.

The objective temperature of the skin—that is, the sensation conveyed to the applied hand of the observer—is a most fallacious guide. I have known the skin to be objectively of normal warmth with a temperature of 105 deg., and cool with a temperature of 102 deg. The use of the thermometer is, therefore, the only reliable method of ascertaining the actual temperature of the body.

As the thermometer has been but little employed hitherto in the diagnosis of pyrexial diseases, a few remarks as to the construction of the instrument, and the proper method of applying it, are necessary before I proceed farther.

The instruments with which I have made most of my observations, I obtained from Mr. Griffin, of Garrick Street, Covent Garden. The scale ranges from 55 to 130 deg., each degree being divided into fifths. Some are curved near the bulb, so as to fit more easily into the armpit; but the advantage thence derived is not great; and I find that the instruments thus made are rather more apt to slip down out of the axilla than those which are straight. The accuracy of the thermometers must be thoroughly established before they are employed, by comparing them with a standard instrument. Dr. Aitkin has arranged a maximum self-registering instrument, which does not require to be read *in situ*, but may be removed and inspected when convenient. Mr. Casella of Hatton Garden supplies this instrument in a box, with a second ordinary thermometer having a bent bulb. Negretti and Zambra also manufacture most excellent instruments. A thermometer, sufficiently accurate for all medical purposes, may be obtained for a moderate sum (seven shillings) of the first named maker (Griffin).

The patient must have been in bed, well covered up, an hour at least before the temperature is taken. If he have been lying on his side, he must be turned round, in order that the thermometer may be applied in the axilla which has been next the bed. He must be placed in a medium position between the side and back ; the bulb of the thermometer passed well up into the top of the axilla ; the arm laid across the chest ; and especial care taken that the instrument be thoroughly surrounded by the soft parts, that no clothes intervene, and that the patient keep his arm in place.

If an approximately accurate observation be all that is desired, the thermometer need not be retained *in situ* more than five minutes ; but it is much better to allow it to remain for ten minutes. The plan which I usually adopt is to note the height to which the mercury has risen at the end of five minutes, and then wait for a minute or so ; when, if I find that a rise has taken place, I allow the instrument to remain in three or four minutes longer. The rapidity of the rise of the mercury, the pulse, respirations, and state of the skin, should be simultaneously observed.

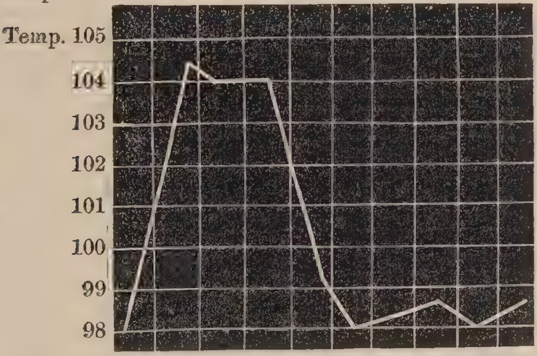
The daily thermometric changes, with the variations of the pulse and temperature, may be conveniently recorded by means of a diagram such as that given on the next page.

Two daily observations, sufficient in the majority of cases, should be made, as nearly as possible at the same time each day, between the hours of seven and nine in the morning and five and seven in the evening. If more frequent observations be deemed desirable, midnight and noon are considered the most advantageous hours.

Range of Temperature in Disease. The height of the temperature is usually a sure indication of the severity of the fever. If, in a case of typhoid, the temperature in the evening during the second week do not rise above 104 deg., the disease may be expected to run a mild course ; whereas, if it amount to 105 deg. in the evening or 104 deg. in the morning, the patient is in considerable danger. In traumatic erysipelas, however, the temperature frequently rises with extreme rapidity to a very high degree, without the supervention of any alarming symptoms. For example, a man aged 60, under treatment for ulcer of the leg, was infected with erysipelas by a patient in the same ward. The attack began at eight o'clock in the morning with a fit of severe rigors; at nine, his temperature was 104 deg.; it remained high till the morning of the third day, when it had fallen to the normal standard. The disease was superficial, and did not spread above the groin. At no time were there any alarming symptoms. Again: a man aged 22, an epileptic, who had two or three small ulcers on his head, produced by tartar emetic ointment, contracted erysipelas. The attack was slight; no danger threatened; nevertheless, his temperature rose to 104·6 deg. In none of these cases, however, was the high temperature of long continuance.

[DIAGRAM.] *Traumatic Erysipelas.*

	1		2		3		4		5	
	M	E	M	E	M	E	M	E	M	E
Pulse......	80	120	120	120	80	68	68	80	60	70
Resp.......	20	20	20	20	24	24	24	24	24	24



When, however, the temperature in any febrile disease approaches 106 deg., the patient is in imminent danger. "A temperature of 108 and 109 deg. indicates most surely the near approach of death." (Wunderlich, who records a case of idiopathic tetanus in which, at the moment of death, the mercury stood at 112·5 deg.; after death, it rose to 113·8 deg.)

The highest temperature which I have observed occurred in a case of pyæmia, in a woman aged 40, who had her forearm amputated for cancerous disease of the wrist. At two in the afternoon of the ninth day after the operation, she had a severe attack of rigors. At half-past three, a temperature of 103·6 deg. was noted. It rose *continuously*—a sign of evil augury in very many cases of pyrexia—until death, which took place thirty-eight hours after the attack of rigors. An hour before death, it had reached 107 deg.

In pneumonia, the height of the temperature gives a much surer indication of the presence and amount of pyrexia than either the rate of the pulse and respirations, or the physical signs. The latter continue in *statu quo*, in many cases, long after the temperature has fallen to the normal standard. The physician, therefore, who judges from these symptoms alone, will be in danger of falling into the error of unnecessarily prolonging antiphlogistic treatment. On the other hand, the temperature is in some cases very high whilst the physical signs afford but slight evidence of the amount of impending disease.

For example, a man, aged 45, with signs of pneumonic consolidation extending as far upwards posteriorly as the sixth rib, had a respiration rate of 32 per minute, yet his temperature was perfectly normal. A woman, aged 19, having similar signs, with pulse 100 and respirations 32, had a perfectly normal temperature. A boy, aged 14, in whom the physical signs consisted merely of slight dulness with feeble bronchial respiration at the base, was found to have a temperature of 107 deg. How, in the first named cases, could the absence of pyrexia have been diagnosed—how, in the last, could the impending severity of the attack (for severe it became) have been prognosticated—without the aid of the thermometer?

Dr. Ringer *(Temperature of the Body in Phthisis and Tuberculosis)* has proved that there is a continuous elevation of temperature in all cases in which a deposit of tubercle is taking place; that the temperature is a more accurate indicator of the amount of disease than either the physical signs or symptoms; that, by means of the temperature, we can diagnose phthisis long before any physical signs or symptoms can be detected; that, by means of the thermometer, we can decide when the deposit of tubercle has ceased; and that, by means of that instrument, we can diagnose phthisis from diseases which more closely resemble it—for example, carcinoma of the lung, aortic aneurism, and dilated bronchi. By means of the thermometer, we can diagnose tubercular meningitis from cerebral tumour.

An abscess lying in front of a large artery sometimes so closely resembles aneurism that the diagnosis is excessively difficult. In every case of abscess which I have observed, the temperature has been above the normal standard—often considerably. An abnormally high temperature has, in not a few cases, led me to the diagnosis of impending abscess for some hours before fluctuation could be detected. The temperature begins to rise at the commencement of the process of suppuration. It rises rapidly until the process is mature, when it begins to fall.

For example, a man, aged 39, was admitted for a severe bruise of his thigh. On the evening of the third day after the accident, his temperature was 103 deg. Hardness and intumescence of the soft parts was to be felt near the seat of the injury, but no fluctuation. A poultice was applied. The next evening, his temperature was 105 deg. The abscess became matured during the night. The temperature fell gradually next day.

In some cases of severe concussion of the brain, it is of extreme importance to establish with certainty the existence or non-existence of any inflammatory symptoms. Mere nervous shock will in not a few such cases cause the pulse and respirations to be very rapid. The following is a case in point. A boy, aged 14, admitted with severe cerebral concussion, had a pulse of 120 and respirations 36. The temperature, however, did not rise above 101 deg., which proved the absence of any severe inflammatory mischief.

In cases of intestinal hemorrhage in typhoid fever, the temperature falls for some hours before the blood appears in the stools, the pulse continuing as rapid, or even more rapid, than before the hemorrhage took place. The thermometer enables the physician to determine the occurrence of the hemorrhage, and to adopt pre ventive treatment at once; whereas if, neglecting the use of that instrument, he wait until the blood appears in the motions, much valuable time will have been lost.

A knowledge of the range of temperature peculiar to each disease enables the observer, when he perceives any marked departure from the type, to pronounce either that his diagnosis is wrong, and that the disease is not what it appeared to be, or that some secondary lesion has supervened.

For example, the typical range or temperature in typhoid (Peyerian) fever, as established by Wunderlich, is as follows.

	Morning.	Evening.
First day	98·5 deg.	100.5 deg.
Second day	99·5	101.5
Third day	100·5	102.5
Fourth day	101·5	104
Second half of week	102	104

At the commencement of the second week, in mild cases, the temperature begins to decrease; for, although the evening temperature may be 104 deg., the morning is only 102 deg. In severe cases, on the other hand, the morning temperature is above 103 deg. ; the evening above 104·5 deg., and may reach 106 deg. In the third week, remarkable vacillations of four to six degrees between the morning and evening temperature occur. The fever terminates by a gradual defervescence *(lysis)*, not by a sudden *crisis* as in typhus. In mild cases, the fever lasts from three to four weeks ; in severe, from four and a half to ten weeks.

Typhoid fever is not present, if, on the evening of the first or second day of the disease, the temperature be 104 deg. ; if the evening temperature do not rise to 103·3 deg. between the fourth and sixth days; if, in the second half of the first week, the evening temperature decrease considerably; lastly, if between the eighth and eleventh days, the temperature be below 103 deg.

As an illustration of the truth of the first proposition, I cite the succeeding case. A woman, in a ward of the hospital in which there were two or three cases of typhoid fever, had one evening an attack of rigors. Her temperature was 102 deg. ; on the morning of the next day, 103 deg. ; in the evening, 105 deg. The symptoms in all other respects resembled those of incipient typhoid. The woman was much alarmed, for a patient had just died of typhoid fever. The existence of a temperature of 105 deg. in the evening of the second day enabled her to be assured that she was not threatened with that disease. This prognosis was proved to be correct by the symptoms developing shortly into those of erysipelas.

The next case is an example of the value of the second and third propositions. A girl, aged 21, was admitted on January 15th, 1866, with symptoms of impending typhoid. The attack began with rigors on the evening of the 11th. She had great abdominal tenderness, a hot dry skin, pains in the limbs, muscular tremors, and one or two suspicious specks on the skin of the chest. Nevertheless, because the temperature did not on any evening rise to 103 deg., and a marked decrease of temperature occurred on the evening of the 18th, the disease was pronounced not to be typhoid. On the 20th, she was quite well.

The next case illustrates the last proposition. A man, aged 40, living in a locality in which typhoid had lately been rife, who had had rigors, followed by diarrhœa and pains in the limbs, a week previous to his admission, was found to have a typhoid tongue, frontal headache, diarrhœa, and numerous specks on his abdomen and chest, very closely resembling the eruption peculiar to typhoid fever. On the tenth evening of the disease, his temperature was 101.8 deg. This temperature forbad the diagnosis of typhoid. In three days after, the man was well. —*British Med. Journal, March* 10, *and* 17, 1866, *pp.* 249, 278.

5.—OBSERVATIONS ON THE PRESENT EPIDEMIC OF TYPHUS.

By Dr. Robert Perry, Physician to the Royal Infirmary, Glasgow.

[The cases mentioned by Dr. Perry were all characterised by distinct and decided febrile symptoms, and were accompanied by decided mulberry rash. Dr. Perry holds strongly to the opinion that the two forms of fever are quite distinct.]

With no desire to detract from the accuracy of the observations of the late Dr. Joseph Bell, one of my predecessors in the Glasgow Royal Infirmary, or from the subtlety of reasoning displayed in that admirable series of papers published in the Glasgow Medical Journal in 1860 and 1861, in which he strives to prove the identity of typhus and typhoid fevers, I must here remark that I have not been able to find in the intestines of patients who have died of undoubted typhus, those pathological conditions of Peyer's glands which he states to be present and identical in both forms of fever. It is true that there is frequently found in typhus a vascularity or congestion of the mucous membrane of the small intestines, and in a somewhat smaller proportion the mucous membrane of the large intestines is also found in a similar condition. We need not be surprised at this state of the intestinal mucous membrane, nor of the turgid condition of the glands at times seen, as hyperæmia of the various organs seems to be the principal pathological condition of typhus, and there would be matter for surprise were the glands never found in such a state.

When the two kinds of fever have been running their course in the same patient at once, then the lesion of Peyer's glands, peculiar to typhoid fever, might naturally be expected.

Although in the present epidemic neither Peyer's glands nor the solitary glands of the small intestines exhibit after death from typhus those lesions, which are now considered as pathognomonic of typhoid fever, I do not consider, on that account, we are at liberty to call in question the accuracy of those writers who have recorded the fact of similar lesions occurring in former epidemics of typhus.

Fifty-two cases were inspected by Dr. A. Anderson in the Glasgow Royal Infirmary in 1837-38, and in nearly all of them he found congestion and enlargement of Peyer's and the solitary glands, with five cases of ulceration in Peyer's glands.

I have seen ulceration of Peyer's patches in one case only out of thirty inspections. In one other case I found all the solitary glands of the small intestines enlarged and infiltrated with a soft yellowish white deposit, which upon examination under the microscope was seen to be of homogeneous character, without any trace of structure or cell development. From the fact of the enlargement or ulceration of Peyer's glands being rarely seen now in typhus, I think we may conclude that the type of the disease is not at all times the same. Adopting the views of pyrexia so ably enunciated by Dr. Parkes, of University College, we may suppose that at times the products of increased metamorphosis of the tissues are retained in the blood, and in the cases where congestion and ulceration of the intestines occur, such retained products are thrown upon the mucous membrane of the small intestines, thus producing a local inflammation there.

In the present epidemic, upon the same theory we may say that the retained products of tissue metamorphosis were thrown upon the pulmonary tissue, as congestive pneumonia proved to be the most frequent and most fatal complication. In the same way I might explain the frequent occurrence of congestion and ulceration of the urinary bladder, observed by myself in 1850 in the Glasgow Royal Infirmary, while acting as assistant to the late Dr. Robert Macgregor. I related to the Pathological Society the history of 25 cases, and exhibited some of the recent specimens, and some casts of the pathological appearances at that time so frequently found in the bladder. The cases were afterwards published by Dr. Macgregor in the London Medical Gazette, New Series, vol. i., 1851. Some of the preparations may still be seen in the pathological museum of the hospital.

With respect to the treatment of typhus fever, it is a trite remark, that after it is once fairly established no man can cure it. All we have to do is to guide our patient through it, treating symptoms and complications as they occur. During the earlier part of this epidemic, from the want of adequate hospital accommodation in the city, the managers of the Royal Infirmary showed an anxiety to provide room for as many patients as possible, and the consequence was that, until the city authorities erected a temporary hospital, the wards of the Royal Infirmary were very much overcrowded.

At present when all the beds in a ward are occupied, an average of 926 cubic feet of space is allowed to each patient. Before the erection of the temporary fever hospital, and when the wards of the Royal Infirmary were crowded, there was only a space for each of about 700 cubic feet; and in some of the temporary wards then opened, the cubic space was very much less than this.

In those smaller and temporary wards the mortality was much higher than in the regular and more spacious ones. It is proper, however, to mention, that the higher rate of mortality in the smaller wards was not altogether attributable to the smaller amount of space, but in a great measure to the fact of many of the most severe cases being sent into them, as those wards were nearest to the reception room; and patients who were in a state of great prostration, or occasionally moribund on admission, were thus saved from the extra fatigue of being carried upstairs.

Dr. Murchison, in his treatise on Continued Fevers (p. 247), states that "500 cubic feet of space, with 2 cubic feet of ventilation per minute, constitute the smallest amount that can be safely allotted to each person. In workhouses the amount of space erected by the Poor Law Board is 300 cubic feet for a sick ward, or for a dormitary occupied by night only; and 500 cubic feet in a ward occupied by day and night." And again, "In hospitals where typhus patients are admitted, there ought to be an allowance of at least 1500 cubic feet to each bed." Although under the present arrangement there is nominally only an average of 926 cubic feet of space for each fever patient in the Glasgow Royal Infirmary, there is in reality a considerably greater space, as it is not a very common occurrence for all the beds to be occupied at one time in a ward, and there is now no reasonable ground of complaint respecting the accommodation.

In a large majority of the cases, the treatment adopted was of the simplest description, and may be said, in a few words, to consist of a supporting diet, with good nursing, and constant and careful watching against the supervention of any complications or untoward symptoms—meeting such complications or symptoms, as they arose, with the treatment deemed most suitable to each individual case.

In order to give a fair trial to the acid treatment, so highly recommended by Dr. Murchison and others, I prescribed the nitro-muriatic acid in a considerable number of cases. The theory upon which the use of the mineral acids has been so strongly advocated, appears to be beautifully simple, viz., to neutralize the poison of typhus, which is supposed to be present in the blood as a peculiar compound of ammonia; since not only is there an increased quantity of ammonia exhaled by the lungs and the skin, but also the urine and the discharges from the bowels become highly ammoniacal.

In the cases so treated, I must confess that I have not met with that very marked amelioration of the disease which many of the writers on fever have ascribed to the administration of mineral acids; and in not a few instances, when given in the proportions prescribed by Dr. Murchison—viz., 20 minims of dilute hydrochloric acid, and 10 minims of dilute nitric acid, every third hour—I have frequently been obliged to discontinue their administration on account of severe griping and diarrhœa.

When the acids are cautiously administered in rather smaller doses, in conjunction with a few minims of solution of muriate of morphia if the bowels be irritable, sweetened with syrup of orange peel, and diluted with water, the draught so formed is generally relished by the patient, and often produces a noticeable effect upon the fever—the most marked improvement being seen upon the tongue, which becomes moist and clean, from being very dry and hard, and covered with a brown fur.

In a few cases I gave small doses of permanganate of potash, in order to ascertain if it exercised any antiseptic influence upon the typhus poison. In two of the cases with a very dark petechial rash, it seemed to hasten the disappearance of the eruption. The tongue also began to get clean and moist at an early stage; but as it produced no apparent diminution of the temperature nor of the pulse, and did not shorten the duration of the fever, and as no good results were observed to arise from its administration in the remainder of the cases, I was not encouraged to give it any further trial.

In No. 51 of the Glasgow Medical Journal, October, 1865, there is a communication by Dr. De 'Ricci, of Dublin, in which he advocates the use of sulphites and hyposulphites in the treatment of zymotic diseases. "Physiology," he says, "leads to believe that all zymotic diseases depend on a fermenting or catalytic principle in the blood. Chemistry teaches us that in the presence of sulphurous acid and the sulphites no catalytic action can take place, and practical experience confirms the teachings of science, by showing us palpably that zymotic diseases, even in their most virulent forms, become completely neutralized by this remedy." This discovery he ascribes to Professor Giovanni Polli, of Milan. I may here remark, that it is much to be regretted that medical men are so frequently in the habit of making such unqualified assertions respecting the use of various remedies. One might suppose that with such a remedy in hand the treatment of fever would become a very simple matter indeed—neutralize the poison, and, as a natural consequence, the fever must cease.

Being satisfied that the bisulphite of soda—the preparation chiefly recommended—is, at all events, innocuous, I determined to test its efficacy in typhus. After carefully watching the progress of at least a dozen cases in which scruple doses of the bisulphite of soda were administered every three or four hours, I am led to the conclusion, that in typhus fever, at least, no amelioration of the disease is produced by it; as to its action in scarlatina, smallpox, and puerperal fevers, for which it is so highly lauded in the paper just referred to, I am not prepared to speak, having had no experience of it in such cases. Had I been so rash as to have given an opinion upon the use of the sulphites without giving them a full trial, I might have fallen into the mistake of supposing them capable of completely neutralizing the poison of typhus, because in one of the first patients who got this treatment, there was a very remarkable fall of the pulse, accompanied with an unusual defervescence or diminution of temperature on the 8th day of the fever—the pulse coming down from 120 on the 7th day to 96 on the 8th, and the temperature at the same time falling from 102·8° to 101°. On the 10th day both pulse and temperature were normal, the former being 74, and the latter 98·2°. Although, on admission, this appeared to be a very severe case of typhus, with dark and copious eruption, still I am not inclined to attribute the unusually early termination to the effects of the bisulphite of soda, but rather to a peculiarity in the constitution of the patient, as I found that after his complete recovery he had a remarkably slow and calm pulse. I was the more confirmed in this view from the fact, that in the adjoining bed to this case was a man whom I had dismissed about three weeks before, after recovery from pneumonia. During the time he was under treatment, for pneumonia, it was observed that he had a very slow pulse, which never rose on any day above 72, whilst the temperature was 100°. He returned with a distinct typhus rash, and other symptoms of pyrexia; and although he had nothing but dietetic treatment, his pulse never rose above 90, and on the 11th day of the fever was only 72. It afterwards fell as low as 48, but the patient by this time felt so well as to wish to be dismissed. In none of the other cases did I observe the smallest degree of benefit from the use of the bisulphite of soda; all I can say is, that while it does not appear in the slightest degree hurtful, it is by no means an agreeable medicine to the poor patient.

The only other point, with respect to the treatment of typhus, upon which I wish to make a few remaks, is the administration of alcoholic stimulants, a subject which has of late been exciting a considerable degree of interest amongst the profession, chiefly owing to the "Facts and Conclusions as to the use of Alcoholic Stimulants in Typhus Fever," embodied in a paper read to the Medical Society of Glasgow, 5th January, 1864, by Dr. Gairdner, and to some remarks upon the same subject, and by the same author, published in the Lancet, January 21, 1865. Several other communications from Ireland and elsewhere appeared about the same time in the public medical journals, in which the writers described the successful treatment of such and such a number of typhus cases without stimulants, indicating that such a line of treatment must be considered somewhat rare or novel.

I have never been in the habit of administering alcoholic stimulants in typhus in the nature of a routine, nor have I been at all guided in my use of them by the age of the patient, but in every instance I judge each individual case according to its own requirements, administering or withholding wine or spirits quite irrespective of the age of the patient, as the particular case requires the stimulant or does not. Practically, however, the conclusions at which I have arrived, from a retrospective review of my cases, are almost identical with those so forcibly and clearly enunciated by Dr. Gairdner.

Owing to the circumstance of a journal of one of the wards having lately gone amissing, which contains the details of a few of the cases, it is not in my power, without an amount of research for which I have not leisure at present, to classify the whole of my cases according as they have been treated with or without alcoholic stimulants. I have, however, ascertained that 534 cases were treated with wine or spirits during some part of their illness, and out of this number 138 died. On the other hand, 491 were treated without any alcoholic stimulants, with only 9 deaths.

From the much higher rate of mortality amongst the former than amongst the latter, I would caution any one against drawing the conclusion, that the greater relative number of deaths was at all influenced by the administration of the stimulants. All the inference that I think may be deduced from those numbers is, that a large proportion, say fully one-half, of the cases of typhus in the present epidemic, may be advantageously treated without any alcoholic stimulants.

Doubtless a considerable number of those who got wine or spirits would have recovered without a drop of either, but I am satisfied that, in many of them, the risk of a fatal issue would have been much increased, and the patients would have had a much more tedious convalescence.

Of 245 patients below 15 years of age, only 29 had any alcoholic stimulants, and several even of that small number had only very small quantities, and for only a few days.

The state of the cardiac and radial pulses, as pointed out by Dr. Stokes, is the best guide for the administration of alcohol; and, as I before mentioned, the time for giving and the quantity required must be regulated by it in each individual case.—*Glasgow Med. Journal, Jan.* 1866, *p.* 407.

6.—RESEARCHES ON THE DAILY EXCRETION OF UREA IN TYPHUS FEVER, WITH REMARKS.

By Dr. Keith Anderson, Resident Physician, Royal Infirmary, Edinburgh; President of the Royal Medical Society.

[The importance attaching to the amount of urea formed and excreted in fevers arises from the prevalent doctrine that fever essentially consists in an increased metamorphosis of tissue, and also from the question how far the comatose or head symptoms of fever are due to uræmia. The only analyses of the whole urine, in cases of typhus are those of one case by Dr. Parkes, and, since the author's own observations were made, of two by Mr. Dunnett Spanton. Dr. Keith Anderson has lately investiated the question at the Royal Infirmary of Edinburgh.]

The general results of the inquiry have been that, *in all of the cases the quantity of urea excreted daily, during the second week, was decidedly below the standard of health*, notwithstanding that the patients were in a state of high fever, with the temperature and the pulse much above the normal rate. These results differ markedly from those of Dr. Parkes' analyses, and are opposed to the belief which many entertain of an increased excretion of urea in typhus. With regard to the daily excretion, during the first week, nothing definite could be made out, as the patients entered hospital at too late a period of the fever to admit of a sufficient number of observations. In three of the cases, however, viz., Cases 1, 4, and 6, the excretion of urea was found to be comparatively high at the end of the first week, but not above the normal amount.

The cases may be divided into two classes; one, in which the crisis occurred on the fourteenth day, while the nervous symptoms continued to some extent for a day or two longer; the other, in which the crisis took place by a process of gradual improvement, beginning about the tenth or eleventh day, and completed about the fourteenth. In the *first* class, an increased discharge of urea took place at, and immediately after, the crisis. Subsequently to the occurrence of this increased discharge, the daily excretion of urea fell to a very low amount, and then rose gradually as the patient improved in health. In the *second* class, the nervous symptoms abated about the tenth or eleventh day, after an increase in the excretion of urea. The completion of the crisis on the fourteenth day was followed by a fall in the amount of urea excreted daily, which then gradually rose with the return of health, as in the other class.

In his Gulstonian Lectures on Pyrexia, Dr. Parkes has shown that the febrile condition is attended by increased metamorphosis of tissue, and in consequence, by an augmented formation of urea. It also appears from his observations that this increased formation of urea is not necessarily followed by an increase in the quantity eliminated, but that in certain circumstances, a considerable amount of urea may be retained in the system. In my cases, such a retention seems to have taken place, since an increase of urea was observed, as before mentioned, either on the occurrence of crisis, or on the appearance of the first symptoms of improvement. That this increased excretion was due to a previous retention, and not to more active tissue-metamorphosis, consequent on the improvement in the condition of the patient, is rendered probable by the fact, that the excretion of urea was greatly lowered after the sixteenth or seventeenth day, and rose in amount exactly in proportion to the rapidity with which the patients regained health and strength; and also by the fact of the excretion having been very low for some time previous to its increase.

It is generally admitted that in Bright's disease and cholera, and in many other affections, stupor, delirium, and other nervous symptoms of a low or so-called "typhoid" character, are due to a retention of urea. In regard to the head symptoms of typhus, a similar explanation has been proposed. In my cases, the fact of those symptoms being most intense while the urea-excretion was lowest, and of their improvement coinciding with, or following on, an increase in that excretion, appears to favour this view very strongly. In two of the cases in which the increased elimination took place on the days immediately following the fourteenth, the nervous symptoms continued to a considerable extent during those days, although the fourteenth had evidently been the critical day. In the cases in which the increased elimination took place before the fourteenth day, no delirium was observed after the occurrence of the increased discharge. The probability of this theory is strengthened by the fact that the other symptoms retained their febrile character, in the latter class of cases, till the ordinary period of crisis.

On the other hand, it may be argued that the retention of urea was merely a consequence of the abnormal condition of the nervous system, and not in any way a cause of that condition. This view would appear to derive support from my cases, in so far as, in the majority of them, the increased elimination did not take place till *after* the crisis had begun; but I think this support is more apparent than real, as, in half of these cases, the nervous symptoms continued to a considerable extent during the occurrence of the increased elimination, and did not finally disappear till the urea had fallen to the low standard natural in the enfeebled state of the patients.

It is very difficult to say why, failing organic lesion of the kidneys, urea should be retained, or why it should be retained to a greater extent in some cases than in others. Perhaps the retention is due to a defective innervation; the theory at present entertained, that in fevers there is a degree of paralysis of the nervous system, gives countenance to such a view.—*Edinburgh Medical Journal, Feb.* 1866, *p.* 708.

7.—ON TYPHUS FEVER.

By H. W. Randolph, Esq., Milverton, Somerset.

[It is generally supposed that whilst typhoid fever owes its origin to badly constructed drains, typhus fever is produced by overcrowding: thus Murchison in his work on Continued Fevers, p. 25, says, "The conditions under which the poison is developed, *de novo*, are overcrowding of squalid human beings with deficient ventilation; in other words, the poison is generated by the concentration of the exhalations from living beings, whose bodies and clothing are in a state of great filth." All the recorded examples of isolated outbreaks of typhus point to this cause. We are therefore somewhat surprised to see an example in which the causes of typhoid produced typhus fever. It would have been well if Mr. Randolph had described the character of the rash.]

As it is still an unsettled question in the profession, whether typhus fever be contagious or the result of an epidemic; whether it be specifically conveyed from person to person, or influenced by atmospheric causes—a poisoned state of the air we breathe, overcrowded habitations, placed over foul cess-pools, or badly constructed drains, I think I shall be enabled to advance evidence to prove that an outbreak of fever in Milverton in 1863 owed its origin entirely to the latter cause.

Situated in the lowest part of Milverton is a nest of houses, which, some twenty years since, was the parish work-house, but is now converted into seven cottages. The families are small, and the rooms capacious; but at the head of the row, turning at right angles with the top house, and facing you as you enter the premises, are several pigsties, and a large open privy for the accommodation of all the inhabitants of the row. The stench from this putrefying accumulation, openly exposed, without the slightest means of escape, was perfectly sickening, and highly offensive to the senses of all but those who were habituated to it. The consequence was that, in the fall of 1863, a fever of a typhoid character broke out, of a very malignant nature. No fewer than twenty were struck down; four died, whilst all the others went through a very protracted convalescence. The fever assumed a low type, presenting no acute symptoms, been ushered in with great prostration. There was no cerebral disease; no derangement of the digestive organs,; no symptoms, in fact, indicating other treatment than stimulants and light nutritious diet.

The only important and interesting feature in the case worthy of attention, is the cause that produced this sudden attack of fever, and how it happened that the inmates of the several house, one after another, without any communication with each other, were smitten with fever. The first case was that of a boy, 11 years of age. He was taken with mild febrile symptoms; and, with all the best care and attention that could be bestowed, the attack ran through the very worst form of typhus, and the boy, though he ultimately made a good recovery, was months under treatment. The case thus cursorily mentioned may be said to represent the type of fever that prevailed throughout the several infected houses.

The first family attacked lived in the house at the bead of the row, contiguous to the pigsties and privy; whilst the second case, which ended fatally, was that of a man in the last house but one of the row, who had had no communication whatever with the family of the first house. The third case was two doors from the second. All the inmates of this house were struck down. Then came others in quick succession, until all the inhabitants of the several houses in the row were prostrated in fever. There was no evidence, from first to last, of any personal contact; and this sudden outbreak, I think, may fairly be attributed to the poisonous atmosphere arising from the causes before mentioned.

As the property belongs to an individual with whom I have some influence, I lost no time in representing the necessity of an entire and immediate removal of the great cause of offence. All the pigsties, &c., were swept away and built afresh far up in the garden. The premises were thoroughly drained, and the houses white-washed. The epidemic subsided; and, as this row of houses is detached by several hundred yards from other dwellings in the place, it was satisfactory to find that no fresh cases presented themselves in the neighbourhood, but that it was strictly confined to this locality; though, had it been contagious, it could not have failed to have been conveyed to other parts of the town by the nurses attendant on the sick.

Knowing that many high authorities attribute the spread of this disease to contagion of a specific character, and as the sanitary measures now being enforced in both town and country are, in my opinion, so essential to the happiness and well-being of the community, I think it desirable that the profession should be in possession of all the evidence that can be procured, to enable them to determine for themselves the simple question, whether the disease emanates from a cause not within the control of man, or whether we may look upon our crowded dwellings, choked-up cess-pools, and exposed decaying vegetable matter, as the fruitful sources of that most wide-spread malady, the typhus fever of this country.

I may add, in conclusion, that since the revolution was effected in the locality referred to, the dwellings made clean, and nuisances removed, not one case of fever or other disease has occurred, and the district is now as healthy as any other part of the neighbourhood.—*British Med. Journal, Nov.* 25, 1865, *p.* 553.

8.—ON AN OUTBREAK OF TYPHOID FEVER WITH SPECIAL REFERENCE TO ITS ETIOLOGY, AND COMPLICATION OF DIPHTHERIA.

By Dr. Donald Campbell Black, Oban.

[Small places are much more favourable for accurate observations as to the natural history of fever, than are large towns, where, fever never being absent, the origin of cases frequently cannot be traced with certainty.]

On the 27th of August last, I saw, in company with a medical friend from Edinburgh, a case which I believed to be one convalescent from enteric fever, the history and then present appearances thus indicating. On the 30th of that month I visited a distinct case of this fever in a different part of the town, and occurring in a person who could not possibly have been in contact with the former case, or otherwise exposed to contagion. I saw this case at the commencement, and my diagnosis was confirmed by the gentleman alluded to. On the 3rd of September I was called to another case at a considerable distance from the foregoing, and to which the above remarks equally apply. Between the last date and the 12th September *seven* new cases had appeared, many of them almost instantaneously; from the 12th to the 20th September *ten* additional cases came under treatment; and in a space of four weeks I had under my care over thirty cases, and my friend Dr. McGillivray a greater number. Thus the occurrence of at least seventy cases of enteric fever in an isolated town, obviously independent of importation, created no small amount of uneasiness, and many and conflicting were the opinions entertained as to the probable cause.

Considering the suddenness of the outbreak; that no fever of any kind existed in the neighbourhood; that rich and poor were promiscuously attacked: that the fever was almost exclusively confined to the best and most airy part of the town; that it occurred during the prevalence of unusually hot and sultry weather, the thermometer ranging from 55° to 80° in the shade, being as high as 93° in the sun,—these consderations, I say, naturally indicated to me a general influence, and, in my capacity of medical Officer of Health, I accordingly suggested to the Commissioners of Police the propriety of examining the tank or reservoir *from which that portion of the town in which the fever prevailed* was almost exclusively supplied by means of a common pump. To this suggestion the authorities at once acceded. I watched the examination. On removal of the covering, a putrescent smell was very intensely perceived. A large quantity of mud, in a state of decomposition, occupied the bottom. The *rose* through which the water filtered was imbedded in this material; and some dead mice, &c., were found amongst the débris. It transpired on inquiry that the cistern had not been depurated for over a period of twelve years; and that within about twenty yards of the cistern, and certainly within its possible soakage, the sewage drain of a large hotel passed, and that this drain could not be water-tight, being of rough *rubble* build. The water had a somewhat milky colour, and, on standing for a night or so, deposited an abundant sediment. By tests which I applied, no organic gases were detected. Unfortunately, no portion of the water was retained for minute chemical analysis; but it was manifest to sight and taste that it was contaminated. Together with this, it may be added that there is no thorough system of drainage throughout the town; there is not an abundant supply of water; and such drains as do exist are only flushed during wet weather—conditions certainly favourable in hot weather to the formation of putrescent gases and the production of disease, but which it is in contemplation to remedy without delay.

It was undoubtedly enteric fever we had to deal with. It unmistakably showed itself in the gradual accession of symptoms, alternate flushings, and chills, pains referred "to the bones," headache, profuse perspiration, great prostration, bowel complication, delirium, and rose coloured spots, well marked, at first, as a rule, faint and disappearing under pressure, but subsequently elevated and unaffected thereby, &c. Nine-tenths of the cases occurred in the part of the town supplied by water as above; and such cases as occurred elsewhere came directly or indirectly within the focus of contagion, or infection. It could not have been imported, the sudden outbreak and general prevalence in a particular locality militating against this assumption. And, lastly, the question of etiology. I submit that it is almost proved to a demonstration that the occurrence of the fever, the contamination of water, the defective drainage, and the unusually hot autumn weather, stand in the irrefragable relation of cause and effect.

On the occurrence of a single case of enteric fever in a family without any subsequent spread.—During this epidemic several such cases were observed; but from those in town it is obvious that no inference could be drawn as to the contagiousness of the fever, as all were alike subject to the same influence. I am aware of only one instance in which a single case occurred in the country, without subsequent spread—i.e., being in the country throughout the whole course of the disease. The patient was frequently in town, and no doubt caught the infection there. I attended a case in town where the patient was, contrary to my express instructions, removed to the country during convalescence. She was progressing favourably up to the period of removal (Sept. 3rd); but from that period a relapse had taken place, and, till within a short time, recovery seemed very doubtful. Great emaciation ensued, and copious hemorrhage from the bowels took place. While in town the patient had severe epistaxis and spitting of blood. In this case, which was quite isolated, no subsequent spread took place. But, on the other hand, cases have occurred in the country in which, doubtless, the infection was caught in town, where the disease did spread to other members of the family, and in which I failed to discover any reasonable cause determining its spread or the reverse. I am at a loss to comprehend how it happens that in some cases enteric fever does not spread from the person affected, and how in other cases it affects only certain members of a family, though all are equally exposed to contagion.

Complication of diptheria with enteric fever.—I have never in the course of my reading, if my memory serve me well, observed allusion to this disease as a complication of enteric fever, nor have I ever before seen it as such in practice. In the course of this epidemic, true diphtheria supervened in *three* cases on the subsidence of the fever proper, if the expression be permitted. None of the cases, however, occurred in my practice. One of them I saw frequently. The whole fauces were covered with white patches of plastic exudation, implicating in all probability the larynx. There was extreme prostration, and subsequently epileptiform convulsions of a violent description ensued. During these fits, there was perfect insensibility, unequal dilatation of the pupils, and total blindness. None but the most sanguine could anticipate recovery. The pulse was extremely feeble, about 150, and suffocation seemed imminent. The strength was assiduously kept up by diffusible stimulants, such as brandy, ammonia, &c., and enemata of brandy and beef essence administered at given intervals, with as much of the same as could be given by the mouth. In the course of time several patches of the exudation material were coughed up, and from that time, to the wonderment of not a few, convalescence went on uninterruptedly. Dr. James Paterson, of Glasgow, saw the case while on a vist to this town, and will doubtless recollect it. I saw the same in another case, not nearly so severe however, which also did well. In the third case, which I did not see, but which was described to me by my colleague as similar in all respects to the foregoing, the result was fatal, convulsions having also occurred before death. Of the identity of this throat affection, I may add, there could be no reasonable doubt.

To epitomize, I have arrived at the following conclusions on the subject of enteric fever :—That it is due to the introduction into the blood of gases, or solid material, resulting from the decomposition of organic matter. That it is attended with excessive metamorphosis of tissue, and very frequently a specific inflammation of the gastro-intestinal mucous track and aërian membrane. That the delirium and head complications correspond in severity to that of the bowel complication. That typhous pneumonia and intestinal ulceration may coexist. That the rational treatment consists in the judicious administration of nutrients, and, in such cases as demand it, the exhibition of stimulants. That local depletion, as by the application of leeches to the region of the lungs, stomach, and bowels, is indicated in certain cases by the spontaneous hemorrhages that frequently occur, particularly in the plethoric. That treated thus, enteric fever is a comparatively mild disease, the mortality in my cases being only one in thirty-five, and in this case death took place on the twelfth day of the fever from sudden collapse, the nervous system having been apparently overwhelmed by the poison; the mortality in the aggregate was seven deaths. And that this fever is *infectious* and *contagious*, or, in other words, communicable through the medium of a contaminated atmosphere, and by direct contact.—*Lancet, Feb.* 17, 1866, *p.* 172.

9.—ON THE TREATMENT OF TYPHOID FEVER.

By Dr. Thos. King Chambers, Honorary Physician to H.R.H. the Prince of Wales, and Consulting Physician and Lecturer on the Practice of Medicine at St. Mary's Hospital.

[The following article is part of a review by the Editor of the Lancet of Dr. Chambers' recent work "Lectures, chiefly Clinical." Out of 230 cases of typhoid fever under his care at St. Mary's Hospital, 109 had been treated on "general principles."]

The remaining 121 were submitted to the author's favourite treatment: strong beef-tea and milk every two hours, together about fourteen pints in the twenty-four hours; twenty drops of dilute nitro-muriatic acid every two hours; the treatment being carried out with regularity by careful nurses. The average mortality under general treatment was 21 in 109, equal to 19½ per cent., or nearly 1 in 5. (This mortality is higher than is usual at special fever hospitals, and about the same as at other general hospitals in London.) Under the second method of treatment, it was only 3 in 121, equal to 2½ per cent., or only 1 in 40. Here are sixteen or seventeen persons more saved in the hundred by this treatment than by that in ordinary use. Dr. Henderson of Shanghai states that by its adoption the mortality of continued fever was diminished from 28 per cent. to 7 per cent.; and Dr. Bishop of Naples is equally convinced by experience of its value in the slow fevers of that climate, although their type is usually very different from that of ours. To make the comparison complete, we should have the means of comparing the results of this treatment with that of the "care" of typh fever without drugs. We find in a *brochure* by Dr. Benjamin E. Cotting, president of the Massachusetts Medical Society, a contribution towards such a comparison. He states that, in the epidemics of 1847-48, he "took care" of over three hundred cases of typhus fever without administering drugs. The cases were taken indiscriminately, including those in a dying state when first seen. The result was 31 deaths in 307 cases, being 1 in 10, or 10 per cent. We are far from believing that the figures of results obtained in one epidemic can be fairly compared with those deduced from another; since it is certain that in different localities and in successive outbreaks zymotic fever varies greatly in its intensity: and that upon locality, type of fever, and constitutional race-characters, differences of mortality may depend greater than those likely to be obtained by varying the treatment. Taking, however, Dr. Cotting's figures as indicating results obtained in a severe epidemic of typhus, they afford material for grave reflection; and while Dr. Chambers' nitro-muriatic and continuous nutriment treatment achieved a great triumph over the old treatment, so far as a simple application of the statistical method to apparently comparable series of cases can show, they have a less striking victory over the simple treatment by cleanliness and nourishment of Dr. Cotter. It is the old problem of art *versus* nature. A comparatively slight advantage is, however, probably all that Dr. Chambers would claim or anticipate, for he studiedly attributes a very large share of success in treatment to continuous liquid nutriment and sponging of the skin; and his whole aim throughout these clinical studies is to show that those physicians are wrong who would represent disease as a positive agency alien to the body, and arrayed against the normal life, which is to be cast out and destroyed; and that he is right who looks upon disease as a deficiency of vital power—a disordered balance of vital functions —something less than life—to be treated by "restoring," not by "weakening" life, as Van Swieten and Boerhaave held, and as Schönlein and other moderns still believe.—*Lancet, Nov.* 25, 1866, *p.* 593.

10.—ON ACUTE RHEUMATISM AND ACUPRESSURE.

By Donald Macgregor, Esq., House Surgeon and Medical Superintendent, Barnhill Hospital and Asylum, Glasgow.

[Mr. Macgregor has lately tried both the treatment of acute rheumatism by blistering, and the arrest of hemorrhage by acupressure. The application of blisters to every joint affected by rheumatism must certainly have much evidence in its favour before it could be ventured on in private practice, as many patients would prefer the disease to the cure.]

In every case of acute rheumatism coming under my care for some time back, I invariably order the application of blisters to all the joints chiefly affected, and with the best results. I have now tried it in a good many cases, and never in a single instance found it fail to give relief.

One case, that of a man who had a severe attack two years ago, laying him up for more than six weeks, with most agonizing pain in his joints and great constitutional disturbance, was lately admitted with an attack similar in all respects, according to the patient himself, to the first. Blisters were at once applied to all the principal joints complained of, both in the upper and lower extremities, giving only a purgative internally. The following day he was free from pain except what was caused by blistering the surface. No bad symptom followed, and in five or six days he was dismissed well.

Here, therefore, was an attack of acute rheumatism similar in every way to a former one in the same patient, which incapacitated him for six weeks, cured in as many days. The heart was slightly affected by the first attack, but was certainly not aggravated by the second. From my experience in all the cases I have treated in this way, I think that blistering is the most speedy and effectual mode of treatment yet adopted for this most painful malady. I find the blistering liquid the most simple and easily applied preparation of cantharides. It may be painted over the surface, however uneven, while there is often a difficulty in keeping the ordinary emplastrum cantharidis in contact with some joints, as the knees. It acts quicker also, which is an advantage in very painful cases.

Acupressure.—It is in one case more particularly that I found this method of arresting hemorrhage very useful, when a ligature could not be used. The case was an amputation below the knee in an adult male, necessitated by phadedenic sloughing of the whole of the soft parts of the leg below the middle, after typhus fever. The tissues were so soft and friable that no hold could be got of the posterior tibial. It gave way, either under the forceps or after a ligature was applied. A common darning needle was passed over it, and at once arrested the hemorrhage.

It was removed on the third day. No secondary bleeding took place, and the stump was completely healed up within a fortnight —one of the best stumps I ever saw.

In such a case as this there can be no doubt of the value of acupressure, and my opinion is that, with a little more experience of its use, it will supersede the ligature in every case where it can be employed.—*Lancet, Nov.* 25, 1866, *p.* 593.

11.—ON BLISTERING IN ACUTE RHEUMATISM.

[The following cases are related by Mr. E. Howard Moore, and exhibit "the rapidity, efficacy, and safety of the plan" of treating acute rheumatism by free blistering. They were treated at the Infirmary of the Bethnal Green Workhouse.]

Case 1.—W. H., admitted February 17. All joints affected, but chiefly the right hip, both knees, and right ankle. To these blisters (acetum lyttæ) were applied, which gave relief immediately the serum was evacuated. Next day the right shoulder and wrist were intensely painful and swollen, and were treated in the same way and with a similar result, which shows the necessity of blistering all joints simultaneously, whether affected much or lIttle. Discharged cured, March 1, without any cardiac mischief.

Case 2.—W. L., admitted March 3, aged 19. Second attack. Rheumatic pains confined to the knees and ankles, but the fever in this case ran very high. Blisters applied to the affected joints as in the other case, with an equally satisfactory result. Discharged cured, March 15, without any heart affection.

Case 3.—C. N., admitted March 21. Blisters were applied to right hip, both knees, and both ankles, as in these joints only was pain felt. Much relief was afforded by the following day, but feeling some pain in right shoulder, he asked to have a blister applied to it. Discharged cured, April 3, without any heart affection.

Case 4.—J. L., admitted April 6. Blisters applied to right elbow, right wrist, and right ankle, and also over the heart's region, on account of a slight mitral regurgitant bruit. Discharged cured, May 1, without any heart affection.

Case 5.—J. B., admitted April 12. In this case the wrists only were affected. Blisters were applied, as in the other cases, above the seat of pain. Next day able to move the joints without much pain. Discharged cured, April 24.

Case 6.—S. J., aged 58, admitted April 28. Joints affected were right hip, right and left knee, and right ankle. To these blisters were applied. The fever in this case was well marked. Discharged cured, May 23, without any heart affection.

N.B.—In all these cases no medicine was given, except in Case 4, when a morphia pill was ordered at night, and in Case 6 a saline mixture as a placebo.—*Medical Times and Gazette, Feb.* 3, 1866, *p.* 131.

12.—CITRIC ACID AS AN APPLICATION IN CANCER.

By Charles J. Denny, Esq., Birmingham.

The following case may be taken as a good example of the facts which I am desirous of laying before the profession :—

Mr. D., an old sailor, aged 70, consulted me in January last for an affection which caused intense pain along the whole side of the face, and great difficulty in mastication. His medical attendants had been treating it as a case of "tic." On examination, I found it to be scirrhus of the tongue, caused most likely in the first instance by a carious tooth. In this diagnosis I was confirmed by Mr. Oliver Pemberton and Mr. Thomason, of this town, and more recently by Mr. Crompton. The disease had extended too much to think of removing it. In every other respect he appeared a strong, healthy man. Life was miserable to him, and all I could do was to palliate. Large doses of morphia, chloroform, chlorodyne, and conium were tried at intervals, with little benefit. He was sinking from bleeding and constant pain. A curious fact is that he could invariably tell some days before the bleeding began, as the tongue at those times felt swollen, and the pain was more intense than usual ; whereas after the bleeding there was little pain for some days. He showed me a paper one day in which citric acid was recommended as an application in cancer. I had no confidence in it ; but at his urgent request I ordered him a mouth-wash, containing one drachm of citric acid to eight ounces of water. He came to me a few days after, and said he felt greatly relieved from its use. When at all suffering he applied it, and the pain disappeared like magic. I may say here that it must be used two days before much effect can be expected. He can now sleep well, and go about his business comfortably, which is that of letter-carrier for a large works. He takes no opiates, and firmly believes as I do, that he has found out the means of "soothing the way to death."

The application may possibly prolong life, in so far as it relieves pain, and so prevents, at least in part, the break up of the constitution consequent thereon. It will be seen that this treatment is applicable in many ways internally and externally. I have tried it in two cases of cancer of the breast and one of cancer of the uterus, and with perfect success in so far as relieving pain goes.

I have purposely avoided any theoretical views on this treatment, which will, I hope, receive an impartial trial on a large scale, and be duly reported upon.—*Lancet, March* 24, 1866, *p.* 312.

13.—ON A CASE OF REMOVAL OF CANCEROUS TUMOUR OF THE ARMS.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

[The patient was sixty-two years of age, and underwent removal of a tumour, undoubtedly cancerous, from the upper arm, on the 12th of December.]

Jan. 9th, 1866. The patient had been going on remarkably well until to-day, the wound rapidly cicatrizing. This morning he complains of loss of appetite, has furred tongue, and pulse 100. Erysipelas has broken out in the arm, which is swollen and painful. Skin of a dusky red from the wrist to the wound, situated near to the insertion of the deltoid muscle. Tincture of sesquichloride of iron is painted over the part affected, and the patient ordered to take twenty minims of the same tincture in infusion of quassia three times a day; the arm to be raised upon pillows.

13th. Better; the arm much less swollen, and the wound again looking healthy.

The instinctive treatment of erysipelatous inflammation when you see the redness of the surface and feel the heat of the part, is to apply cold to it. But I would say to you, "Never use any really cold application, which will lower the temperature of the part, as you would by permitting a stream of cold water to trickle over it, or by putting on a bag of ice." But some of you are mentally exclaiming, "Oh, I have often seen cold lotions applied in erysipelas, and do good." I ask you whether it is not a fact that these cold lotions soon become warm applications by contact with the skin? At any rate, my advice certainly is that you never apply cold to cases of erysipelas.

Erysipelas must be regarded as a general blood-poison. Like all blood-poisons, it attacks those parts of the body which are the deteriorated portions of the system, causing an evolution of the poison either into them or upon their surface. But some one will observe, "Is it not much more likely that the erysipelas starts from the wound itself and spreads to the neighbouring tissues, seeing that the margin of the wound is almost always the part first attacked?" I reply that the margin of the wound is the most deteriorated part; hence, erysipelas there manifests primarly its local action. This man was fortunate, inasmuch as the erysipelas attacked the skin of his arm in preference to to manifesting its local action on some of the deeper tissues; it was certainly better for him that an evolution of the poison should take place on the surface than than he should have pneumonia, or that his brain or liver should become disordered. Had a bag of ice been applied to his arm, probably the poison might have been diverted to some other locality, where its attack would have been attended with more serious consequences. I think it right, when erysipelas has commenced on the surface, to solicit its evolution by warm fomentations, medicated with poppy or hemlock, so as to reduce the sensibility of the skin, for an erysipelatous part is usually hyperæsthetic.

But a question naturally suggests itself for our inquiry: If the generation of the poison does not start from the wound, where does it commence? Most frequently I imagine in some part of the alimentary canal. Take this man's case. Just before the erysipelas showed itself, he had loss of appetite, which you will find one of the most frequent premonitory symptoms. He does not seem to have experienced a desire to vomit, though this is often superadded to the loss of appetite. Does not this appear to indicate that the stomach is desirous of getting rid of something which is irritating the organ? I so interpret it, and act upon the suggestion by giving an emetic, and afterwards clearing out the bowels by a purgative. The treatment I have over and over again seen quickly followed by the most beneficial results, the erysipelatous blush quickly disappearing from the surface, the appetite returning, and the patient expressing himself as feeling much relieved by the vomit. But why does the erysipelas cease? I must assume that there is a morbid fluid secreted by and within the stomach, which is reabsorbed by the stomach or intestines, and which poisons the blood and gives rise to erysipelas. I can give no positive proof of the truth of this statement; but I am sure that, by receiving it as true, and adopting as the earliest step the treatment by an emetic and a purgative, you will frequently in your practice be gratified by witnessing the rapid arrest and disappearance of existing erysipelas.

I just now laid it down as a principle that in constitutional disorders dependent upon blood-poisons the local symptoms are manifested at the spot where the tissues are most deteriorated. You see this expressed in the sequelæ of small-pox, measles, scarlet fever, typhus fever, &c. A patient apparently previously healthy goes through the fever process, and just when about to get up, some local manifestation of mischief is discovered, such as inflammation in the knee or at the hip-joint, an empyema, a large chronic abscess forming at some part, &c. This is the more remarkable as far as the joints are concerned, because the patient may have been lying down during some weeks, and the joints have been comparatively free from any disturbance as regards motion at the part. But what is the explanation of the events? The hypothesis is that, previous to the occurrence of the fever, a slight strain or bruise was received at the part, or that from some pre-existent latent cause of deterioration in the structures, these parts subsequently manifest locally the general morbid condition of the system. If a small principle like this can be recognised, you may see how widely it is capable of being applied, and it will help to explain many points which might otherwise be clothed with the obscuring title of "anomalous" or lost in the meaningless term of "caprice of nature."

The skin of the patient's arm was of a dusky-red tinge, and the inflamed part was very little swollen or raised above the level of the surrounding healthy integument. These local symptoms marked the erysipelas as of an asthenic character. His vital powers were low; he had to repair a considerable wound; so that the local inflammation was of an asthenic type. The inflamed parts were painted with tincture of the sesquichloride of iron. Why so? It may act as a direct astringent, or it may excite the capillaries to contract, and thus tend to diminish capillary congestion. Hence it is one mode of applying pressure to the bloodvessels which are dilated, as you can see by the redness of the part. It moreover unites with the cuticle, shrivels it, and forms a thick covering to the inflamed skin, and, acting as a bandage, makes pressure upon the capillaries, and keeps off the oxygen of the air that otherwise acts as a stimulant to the capillaries of the surface. I did not give this man an emetic, because he did not complain of a feeling of nausea; but I ordered him the tincture of sesquichloride of iron internally with infusion of quassia, so as to improve his appetite and keep up the general powers of his system. Or did the steel act upon the contents of the stomach as an antiseptic, and so act beneficially?—*Lancet, Feb.* 24, 1866, *p.* 196.

14.—POLYDIPSIA—TREATMENT BY LARGE DOSES OF VALERIAN—IMPROVEMENT.

Case under the care of Dr. Reith at the Aberdeen Royal Infirmary.

[The patient, who was 24 years of age, was admitted for typhus, but no symptoms of that disease were discovered, and the case was regarded as one of febrile catarrh. The nurse however called attention to an insatiable thirst of which the patient complained, and to the enormous quantity of urine daily voided by her. The patient said that she had suffered from these symptoms for the previous nine months, since her bady was weaned. Both her father and brother had had diabetes; the brother died of it, but the father died of comsumption. The patient's urine was of sp. gr. 1000, and contained no albumen or sugar. She was ordered ℨi. of tincture of valerian three times a day. The spec. gr., however, remained 1001. The powder of valerian was then given instead of the tincture, and during eleven days the daily dose was increased from 60 grains to 140 grains, and the spec. gr. rose during the same period to 1006.]

I lost sight of the patient, owing to an attack of illness, but I was told that before she left the Hospital the specific gravity of the urine had risen to 1008. I afterwards had a sample of the urine sent me on March 3, 1865 ; the specific gravity was 1006. I have not seen the patient since.

Clinical Remarks.—Polydipsia, once established, is generally incurable. Although at first a mere inconvenience, and not interfering materially with the general health (the subjects of it, like our patient, being often stout and ruddy) it, sooner or later —it may be after the lapse of years—tells on the system, and at last it ends in tubercular phthisis. The present case will thus terminate, for there were symptoms of tubercular deposit in the right lung during her stay in hospital. This case is an example of the undoubted connexion existing between glycosuria and polydipsia. Two of the patient's near relatives were affected with the sugar disease, and although the father was said to be cured, it is evident that the phthisis of which he died must have been induced by the glycosuria of which it is so often the termination. The polydipsia, therefore, in this patient, as in most, if not all, cases of the same kind, had come by hereditary descent, so to speak, from glycosuria. In fact, it is now generally admitted that glycosuria, polydipsia, and even albuminuria are interchangeable ; that is, if an individual be affected with one of those diseases, his descendants are liable to either of the others. The well-known experiments of Bernard show that these diseases may be produced by injuring certain parts of the nervous system in close proximity to each other. Hitherto, polydipsia, or, as it is more commonly called, diabetes insipidus, has been regarded as not only incurable, but also irremediable. M. Rayer was, I believe, the first to point out that valerian administered in large and rapidly increasing doses was a most serviceable remedy. Trousseau relates some cases which were perfectly cured, at least for the time, by this treatment. The result of its employment in the present instance was satisfactory, and there is reason to believe that had opportunity been given to persevere in its use, and had the patient been more tractable than she was, she would have been still further benefited.—*Med. Times and Gazette, March* 24, 1866, *p.* 310.

15.—ON THE TREATMENT OF DISEASE BY OXYGEN.

By Dr. R. H. Goolden, Sussex Gardens, Hyde Park.

[The following paper gives the result of some experiments which have been carried out by Dr. Goolden, at St. Thomas's Hospital.]

I had remarked that the effect of the Turkish bath in cases of gout was not so satisfactory as in cases of rheumatism, though I must admit that in many great relief was given by it. The acute paroxysms were relieved for a time, but I could not but remark that the lithates continued to abound in the urine, and not unfrequently in greater excess. This may be accounted for by the diminished quantity of the secretion. This lithic acid condition of urine appears to me to be more prevalent in cases where, from lung-disease or from vitiated atmosphere, the respiratory functions were imperfect, as well as in cases of degenerating tissue, abscesses, and fevers.

Lithic acid does not appear in the cutaneous excretion of the Turkish bath, even where it is known to abound in the blood, and it occurred to me that the formation of urea, instead of uric or lithic acid, might be determined by supplying oxygen.

The idea of inhaling oxygen was mooted by Sir Humphry Davy and by Dr. Eddoes in the year 1804, but not adopted generally, probably from the difficulty of obtaining the gas in sufficient quantity and purity. This difficulty has now been happily overcome, and the means of procuring it in a portable form is afforded us by Mr. Barthe, of Long-acre, and also by Messrs. Garden and Robbins, of Oxford-street, by the manufacture of the oxygen water by the former, and of the binoxide of hydrogen by the latter. For the method of obtaining this latter preparation in its present perfection I believe we are indebted to Dr. Richardson.

Till I could obtain an instrument for inhalation in the hospital, I commenced with the use of the two latter remedies. The binoxide of hydrogen I exhibited in one one-drachm doses, diluted in about two ounces of water, and in many cases I found it to have a marked influence on the biliary secretion, of which it increased the quantity and improved the quality, often producing excessive biliary dejections, and thus relieving congested livers and secondary bronchial congestion. I procured a large vulcanite bag with a tube, stop-cock, and mouth-piece, which was filled with a mixture of oxygen and air in the proportion of one to four. The gas is inhaled by the patient, and expired into the atmosphere. The result has been pretty uniformly to clear the urine, and in many cases of chronic gout the continued use for half an hour daily, very slowly inspired at intervals, filling the lungs as much as possible, has given very great relief, and in some cases cures have resulted.

I have adopted this plan without the use of any medicine or other appliance which might complicate the treatment; but latterly I have found, in carefully selected cases, the Turkish bath a great expeditor of the absorption of enlarged joints, and that in cases where the Turkish bath alone had previously failed.

There are, however, two cases that are particularly deserving attention. The first was that of a married woman admitted under my care in August with phagedenic (and as I thought syphilitic) ulceration of the soft palate. A large opening existed between the mouth and nostrils, so that she could not swallow fluids but with the greatest difficulty. The sides of the ulcerated and divided palate were swollen and purple, and the edges everted, and secreting an unhealthy pus and ichor. There was no history of syphilis. Aware of the benefit in these cases of the inhalation of the fumes of peroxide of mercury when thrown on plates of hot iron, also that mercury in any other form is usually injurious, and considering that such fumes were those of metallic mercury and oxygen, and having the oxygen bag at hand, I caused her to use it twice daily. The result was a very rapid improvement, and in the course of three weeks she left my care, still with a divided soft palate and loss of uvula, but the edges of the opening were perfectly healed and the throat healthy. I may remark that no other remedy was used. The result was beyond my expectation, and I determined to try it in the next case of phagedenic ulceration that should present itself.

This opportunity soon occurred in the case of John M., aged 28, admitted on the 28th November, 1865. The patient had several large ulcers on rose-coloured and swollen bases—covered with brown crusts in the centre, with elevated and everted edges, from which was secreted an unhealthy ichorous discharge—one large one on the vertex, another on the forehead, two on the left shoulder, with many cicatrices, the marks of former sores, and his forehead much disfigured. But he was also suffering from double pneumonia, coughing up quantities of rust-coloured and viscid mucus; the bases of both lungs solid, and crepitations over the other parts of the chest where the air entered; great dyspnœa. The smell of the patient was so offensive that he had to be removed to a separate room, and it required some effort to approach him for examination. The course adopted was to remove, if possible, the stench, for which purpose we used the solution of chloride of lead, formed by mixing solutions of nitrate of lead with common salt (by far the best and cheapest deodoriser, and long in use in our wards). His mouth was the feeling of numbness slighter. He went on favourably until March, 1861, when, having a long and very laborious journey washed with a solution of Condy's ozone water constantly. He took one drachm of binoxide of hydrogen in two ounces of water three times daily, and inhaled from the oxygen bag twice daily. I employed no other remedy, but ordered wine and brandy, and anything he could be induced to eat. This was on the 29th of November. My next visit was on the 2nd of December, when I hardly expected to find him alive, but I found him sitting up in bed, and very cheerful. The lungs had recovered themselves. He took his food with a good appetite. No smell whatever about him, and the ulcers had all assumed a healthy character. He continued these remedies, and no other, till he left the hospital on the 27th of December, the scabs only being removed by poultices, and the sores then dressed with simple dressing. —*Lancet, March* 10, 1866, *p.* 270.

DISEASES OF THE NERVOUS SYSTEM.

16.—ON PROGRESSIVE LOCOMOTOR ATAXY, OR WASTING OF THE POSTERIOR COLUMNS OF THE SPINAL CORD.

By Dr. Julius Althaus, Physician to the Royal Infirmary for Diseases of the Chest, City-Road.

[This disease, recently described by French physicians as progressive locomotor ataxy, was first described more than thirty years ago as tabes dorsalis, yet the nature of the complaint and the connection which exists between the symptoms manifested during life and the structural changes found after death have, until quite recently, been misunderstood.]

Tabes dorsalis is first spoken of in the works of Hippocrates, and was by the father of medical literature believed to arise from excesses in sexual intercourse, the chief symptoms of the disease being spermatorrhœa, marasmus, and hectic fever. This meaning of the term, however, has gradually changed, and those authors who wrote on tabes in the first decennia of this century understood by it atrophy of the posterior portion of the spinal cord, brought on, not merely by sexual exhaustion, but also by exposure to wet, rheumatism, gout, and other causes, the chief symptom being a peculiar form of paraplegia. The disorder was chiefly investigated by English and German physicians, such as Abercrombie Hufeland, Steinthal, Romberg, and others. Their descriptions, although in some instances most eloquent, were however, to a certain extent, wanting in accuracy, inasmuch as several different affections of the cord were comprehended under the names of tabes, and a clear distinction was not drawn between tabes and paraplegia. It was only after a more careful clinical study of the symptoms had been made, and after pathological anatomy, aided by the microscope, had stepped in, that a peculiar disease of itself, and one characterized by uniform structural lesions, could take its place in our nosological system. The chief credit of the anatomical investigations is due to Professors Virchow, Türck, Rokitansky, and Leyden, and in this country to Dr. Gull and Mr. Lockhart Clarke, who have shown that, in well-marked cases of tabes, an actual waste of nerve-fibres of the posterior columns of the spinal cord takes place, together with the formation of amyloid corpuscles and considerable proliferation of connective tissue.

The first who drew a distinction between this disease and paralysis was Dr. Todd. He said, in an article on the nervous system in his Cyclopædia, that two kinds of paralysis might be noticed in the lower extremities; the one consisting simply in the impairment or loss of voluntary motion; the other distinguished by a diminution or total absence of the power of co-ordinating movements. In the latter form, while considerable muscular power remained, the patient found great difficulty in walking, and his gait was so tottering and uncertain that his centre of gravity was easily displaced. In these few words we have a good description of the system of *ataxy*, upon which lately so much stress has been laid by French physicians. The term "ataxy" is as old as that of "tabes" for it also originated with Hippocrates; and it has likewise entirely changed its meaning in the course of time. Some authors have applied it to chorea, others to fevers, others to various nervous disorders. At present however, we understand by ataxy, not a disease of itself, but merely a symptom to which various disorders may give rise, and which essentially consists of a want of co-ordination of voluntary movements, and a tendency on the part of the patient to lose his balance, but without actual loss of power, and apart from tremor, chorea, and paralysis. This symptom may be observed in disease of the cerebellum, and in poisoning by alcohol, lead, and mercury; but it is more especially connected with that disease which has been long familiar to us as tabes. The best clinical study of this symptom we owe to M. Duchenne de Boulogne, who from 1858 up to this time has published a number of memoirs, in which he described what he thought to be an entirely new disease, which he called "progressive locomotor ataxy," and which he believed to be a functional disorder of the cerebellum. His apparent discovery was hailed as a real one in France, and Professor Trousseau actually proposed to call the new stranger "Duchenne's disease;" but, on looking more closely into the matter, we find that Duchenne's description is altogether applicable to our old friend, tabes. I have not the slightest hesitation in acknowledging the great ability and originality of M. Duchenne's researches, which were perhaps more strikingly displayed in this case just on account of his being unacquainted with the previous literature on the subject; yet, if I thought it desirable to attach a proper name to this affection, I should prefer calling it "Todd's disease," as Todd first drew the distinction between ataxy and paralysis eleven years previous to Duchenne. But the best plan is, perhaps, merely to drop the term "tabes," as being too vague, and to call the disease under consideration "progressive locomotor ataxy, or wasting of the posterior columns of the spinal cord."

The following details of a case of this affection which I have had under my observation for about five years are well suited to illustrate the symptoms and progress of the disease:—

R. B., a commercial traveller, aged 37, a tall and rather spare man, with a sallow complexion, first consulted me in February, 1860, when he gave me the following history. His mother had always been healthy, but his father had for the greater part of his life suffered from epilepsy, and died in a fit. The patient was brought up to the law; but in consequence of a disappointment he left that profession, and enlisted as a soldier. He served in Australia and Canada, and during that time suffered much from rheumatism. He never exceeded in drinking, but occasionally in sexual intercourse. He twice had gonorrhœa, but no syphilis. He suffered for a long time from hemorrhoids, for which, in 1855, he underwent the operation by ligature, and he ascribed the commencement of his present illness to that operation, saying that he never felt quite the same man after the hemorrhoidal flow had ceased. He left the army in 1856, and married. He is now father of a healthy child. For the last eighteen months he has been a commercial traveller, and as such he is always on the move, and frequently exposed to cold, damp, and great fatigues. In January, 1857, he first noticed that his sight became weak, and he had some difficulty in writing and reading small prints Soon after he felt pains of a peculiar character, which he described as electric shocks through the legs, and as if the muscle. were being rent asunder. These shocks came every two or three minutes. He underwent treatment by liniments and other external applications, but without relief. His gait now became tottering, and he had considerable difficulty in walking. He never goes out without a stick, and sometimes he is obliged to use two. In summer 1859 he consulted the late Dr. Todd, who told him that his case was incurable, and that he would have to be on the sofa for the rest of his life. He prescribed strychnine and iron, and after having taken it for some time the patient felt rather stronger, but there was no improvement in the special symptoms of the disease.

Present state.—February 14th, 1860: The patient's intellect, memory, and speech are quite normal. He does not suffer from headache, giddiness, strabismus, or ptosis. Both pupils are enlarged, the left more so than the right. He complains of weak sight, and the opthalmoscopic examination shows the arteries of the optic nerve smaller than usual. His senses of hearing, smell, and taste are natural. Respiration and heart-sounds normal; pulse at the wrist rather feeble, but quite regular. His digestion is tolerably good. Tongue clean, appetite satisfactory, but bowels rather costive. He complains of a sensation as if a net were tightly drawn round the abdomen. He is occasionally troubled in passing the urine, but there is no stricture. The urine is of 1030 specific gravity, and contains a sediment of urates, but no excess of phosphates, and no albumen or sugar. The sexual power has not notably diminished of late. On examination of the back by pressure, percussion, galvanism, and hot sponges, no place can be discovered which is particularly sensitive. The patient complains of numbness in the hands, more especially in the third and fourth fingers of the left hand. He can distinguish heat and cold, and feels the prick of a pin and pressure distinctly; yet the æsthesiometer shows a considerable diminution of tactile sensibility in the fingers. The upper extremities are pretty well nourished, and the muscles answer well to a galvanic current of moderate power. He can bend and stretch the arms with force, but he finds it difficult to button his shirt and to feed himself. The lower extremities are more affected than the upper ones. The patient has had sensations of "pins and needles" in the feet, but these have for some time past given place to numbness. He says that in walking he has a sensation as if the steps rose under his feet. He must continually look at his limbs in order not to lose his balance, and can scarcely walk at all in the dark. If told to shut the eyes or stand with both feet together, he begins to stagger. In walking, he throws the legs forwards with a jerking motion; and, as he cannot measure his distance from the ground, he puts his foot down with great force. If lying down, he can bend and stretch the legs with considerable power, but he seems to exaggerate every movement, all muscular contractions being, not slow and equable, but violent, sudden, and jerking.

I ordered the patient thirty minims of the syrup of iodide of iron three times a day, sulphur baths twice a week, and a pill of aloes and myrrh at bedtime. After having used these remedies for about a month, there was a marked improvement in every respect. The pains were nearly gone, the sight was rather better, the walking decidedly steadier and less fatiguing, and to perform, he returned considerably worse, and from that time the disease gradually gained upon him, in spite of all treatment. In 1863 he had a course of nitrate of silver, but the affection was then evidently too far advanced for any medication to do good. The sight got rapidly worse, and he became at last completely amaurotic. The sensation in the lower extremities was also entirely lost. The patient was now confined to his room, and during the last six months of his life he never left his bed. His intellect remained unimpaired to the last, and his disposition was always cheerful. He died in January, 1865, of a sharp attack of bronchitis.

[The author then describes in detail the anatomical changes which are found in patients who have succumbed to this disease; after which he proceeds to an elaborate analysis of the special symptoms, laying chief stress on the affection of the cerebral nerves and the pains in the first stage, and on the symptoms of ataxy and the loss of the different kinds of sensibility in the second stage of the disorder. The causes generating ataxy are then mentioned, and the diagnosis is fully gone into. He then continues:—]

The prognosis is not favourable, for up to the present time not a single case is on record in which perfect recovery has ensued. Indeed, as late as 1851, Romberg wrote that there was no hope for patients of this class, that a fatal issue was inevitable, and that it was but common humanity to inform them that therapeutic interference could only injure. This sad confession of impotence need no longer be made, and we may certainly congratulate ourselves on having far more control over the disease than we had formerly. Moreover, its progress is seldom rapid, although acute intercurrent disorders, such as erysipelas, bronchitis, and pneumonia, are grave complications which may carry the patient off suddenly. Much must depend upon the period at which the case comes under treatment. If all the symptoms of the disorder are fully developed, the hope of a cure may be slight, although even then much may be done to alleviate the patient's suffering. The case is different if the patient presents himself in an early stage of the disorder. The fact that the cerebral nerves, with the exception of the optic, generally recover from their affections in the course of the disease, goes far to prove that previous to the structural changes in the cord there is a functional stage, in which much may be done by medicines. Moreover, we must bear in mind that Messrs. Charcot and Vulpian have found nervous fibres in the process of reparation in the cord of a man who had died from ataxy; and that therefore even at a later time we must not give up hope altogether, especially if the patient is placed in favourable circumstances.

I now come to the treatment of ataxy, and will first say a few words about diet and regimen. This must depend a good deal upon the condition in which the patient is at the time he comes under treatment. If he is in a weakly state, plain and nourishing diet, with iron, quinine, and cod-liver oil, should be prescribed. I have never seen a case in which lowering did any good. Hippocrates has recommended milk diet in erotic tabes, and Eisenmann speaks highly of the same in progressive ataxy. I have often given milk-and-brandy, two and even three times a day, with decided benefit to the general health; but have never found it expedient to insist on an exclusive milk diet. Exercise should be very moderate; and for those who have undergone great fatigue, rest is most beneficial.

Counter-irritants to the spine have been used by many physicians, but, as a rule, the benefit obtained has not been proportionate to the sufferings inflicted by their application. The moxa, the hot iron, issues, blisters, and leeches have been employed. I give the preference over all of these to the continuous galvanic current, applied to the lower and middle portion of the spine. It has in several cases seemed to me of decided benefit in lessening the pains, and also the disagreeable feeling of constriction which is often felt at the abdomen and the chest. The action of this agent is as rapid as that of the iron; its application entails far less trouble and suffering than the latter; and its efficacy is superior to that of issues, blisters, and leeches. Dry cupping on both sides of the spine is also useful.

Iodide of potassium has been recommended by Duchenne and others; but no cases have been published in which this remedy has proved successful. On the contrary, several are on record in which it seemed to accelerate the progress of the disease. I have given it in two cases, but without any effect, and am certainly not in favour of its administration. Iodide and bromide of iron are useful, but do not effect a cure. Mineral waters have been very frequently employed, both externally and internally; and it seems that for a time they do good. Amongst the French spas, Barèges is one of the most recommended; amongst the German waters, Marienbad and Wiesbaden have a special reputation; while chalybeates and indifferent thermal springs, which often prove useful in certain forms of paralysis, have entirely failed in ataxy. On the whole, I should be averse to sending atactic patients on a journey to some distant spa, as rest at home, with certain remedies to be mentioned hereafter, is more beneficial than all the mineral waters in the world. If, however, there should be costiveness and abdominal plethora, Carlsbad, Marienbad, or Kissingen water may be drunk at home with benefit. Sulphur baths may also be taken at home; and I think highly of them. I have never employed them alone: but they certainly seemed, in conjunction with other remedies, to do much in relieving the pains and diminishing the numbness. The patients feel more brisk and supple after the baths, and are almost always desirous of repeating them as often as possible. A sulphur bath may be prepared by dissolving from three to six ounces of the sulphuret of potassium in two pints of warm water, and adding this to the bath. Vapour baths and Turkish baths should be avoided.

Nux vomica and strychnine have frequently been used, but generally seemed to do harm. Duchenne reccommends faradization, and the late Professor Remak, of Berlin, praised galvanization as a curative agent. From faradization I have never seen any benefit; galvanization has proved useful at my hands not only in the pain and feeling of constriction, but also in the affections of cerebral nerves which are so frequent in the commencement of ataxy; on the other hand, it has been powerless against the disease itself, more especially against the symptom of ataxy.

Amongst the other remedies which have been used in this affection without much result, I will mention opium, bromide of potassium, secale cornutum, essence of turpentine, and arsenic. The only remedy which seems as yet to have some good, in a very large proportion of cases, is the nitrate of silver, given in doses of one-tenth to a half grain, two or three times a day. Professor Wunderlich, of Leipzig, was the first who employed silver in seven cases of this disease, in none of which, it is true, he obtained an actual cure, but in most of them considerable improvement. In 1862, Messrs. Charcot and Vulpian, in France, took up Wunderlich's idea, and used the nitrate in five other cases, and in each of them there was much amendment. Since then this remedy has been employed in most cases of ataxy, and with somewhat variable success. In some it has so disagreed with the patients that it was necessary to discontinue its use; in others it had little or no effect; while in the majority of cases the remedy has proved, if not curative, at least very useful, and it is the one upon which most reliance can be placed in the treatment of this disease. I am in the habit of giving the silver combined with the hypophosphite of soda, and it seems to do far more in this combination than either of these remedies singly. I have, indeed, now a case of ataxy under my care in which the improvement has, for the last six months, been so considerable under this medication that I am hopeful of a cure. Certain precautions should, however, be taken in administering the nitrate. I generally employ it for four or six weeks consecutively, and then discontinue it for a fortnight or three weeks, giving in the meantime a slightly aperient mineral water. After this the use of the remedy may be safely recommenced, and continued for a month or so. The gums must be inspected from time to time, as the peculiar coloration which silver produces in the long run first appears in the mucous membrane, and only afterwards in the skin. With the precautions mentioned, however, no disfigurement of the patient need be feared. I have never gone beyond the dose of half a grain, and perhaps this is another reason why in my cases the remedy has been borne without any inconvenience. I should, however, not recommend all cases of ataxy to be treated alike; in this affection we must, as in every other, study each individual case by itself, and advise for it what seems, under the special circumstances, most likely to do good. Thus hysterical or hypochondriacal patients in whom ataxy may supervene, will require a different treatment from plethoric persons, or such as have long suffered from rheumatism, or have been subject to privations and anxiety. Much is, therefore, left to the tact and discrimination which, together with knowledge, should, in all cases brought before him, guide the doings of the physician.—*Lancet, Dec.* 30, 1865, *p.* 726.

17.—A CASE OF PROGRESSIVE LOCOMOTOR ATAXY.

Under the care of Dr. Johnson, at King's College Hospital.

[The disease known as locomotor ataxy has been much discussed within the last few years by Duchenne and others, under the name of "ataxie locomotrice." This disease had until recently been included in the class Paraplegia; but in well marked cases there is no actual paralysis of the muscles, but loss of power to guide them. The patient is obliged to "lean on his eyesight as on crutches," and if he shuts his eyes he stops, and sometimes falls. The posterior column of the cord is the part diseased. The following is the account of a case in a man forty years of age. He had from boyhood been engaged in the cod-fishery off Iceland, and thus for the greater part of the year was much exposed to wet and cold. The symptoms of the disease dated four years back.]

On admission, he was noted as a short, well-built, powerful man; no loss nor thickening of speech, and no deafness. He seemed to be intelligent, and gave a clear account of himself. The most characteristic point was his walk. He could stand very well with his feet apart, but when he put them close together he could not maintain his position, even when looking on the ground. On taking a step, he would advance in a hesitating, tottering manner. He always leaned forwards, and seemed to have some difficulty in balancing himself, especially when turning round. These points were better seen when he shut his eyes, for he was only able to totter a few yards, and then would fall. He walked pretty well when his eyes were off the ground, as when he was looking at the ceiling; but directly he closed his eyes he seemed to lose all confidence, and reeled like a drunken man. Although there was this loss of power in co-ordinating his movements, there was no loss of muscular power. He would lift heavy weights, and even carry patients round the ward. When sitting in a chair with bent knees, he could resist all attempts to straighten the legs. There was no muscular atrophy, and but slightly diminished sensation, in the left hand. The left side was rather more affected than the right. It appears he had a chancre four years ago, but it came on after the first appearance of numbness in the leg. There were some syphilitic patches on the tongue. There was no sore-throat nor rash on the skin; no nodes, nor glandular swellings anywhere. Lung and heart sounds healthy. Urine pale; specific gravity 1010; no albumen. Does not pass urine more than three or four times a day. His intellectual faculties do not seem impaired; he has slight loss of memory. His sight is not quite so good as it was. He has no pains in his head nor down the spine. Hearing and speech good. No affection of the cranial nerves apparently. Appetite good. Bowels open; no loss of power over the sphincters.

He was put on a liberal diet, and treated with iron and quinine. Galvanism was applied daily, with apparent benefit.

During his stay in the hospital he seemed to improve a little. At times he would walk fairly well, but on wet days he was always worse, and depression of spirits would come on. He was discharged Feb. 8th, 1866.—*Lancet, March* 31, 1866, *p.* 344.

18.—A CASE OF PROGRESSIVE LOCOMOTOR ATAXY.

Under the care of Dr. Hughlings Jackson, at the London. Hospital.

[Dr. Jackson observed that this was one of the most interesting cases of locomotor ataxy that he had ever seen. The patient was 39 years of age. The first symptoms observed, when 14 years of age, was loss of sight; but this improved so that in nine weeks he was able to see tolerably. He then became almost quite deaf, and then the sense of smell was lost. Taste has never been affected. There was also clear evidence of diminished sensation in the legs. The patient is liable to sudden and violent pains in the legs.]

The patient can get along when his eyes are open; but his gait his precipitate, and he staggers. When he shuts his comparatively good eye, he can only manage to keep his legs for a moment. It is scarcely right to say that he can stand; a fairer description would be, he does not fall at once. He cannot stir a step with his eyes shut. Dr. Jackson particularly drew our attention to the fact that this patient could manage to walk when looking at the ceiling, although, of course, not so well in so awkward a position. He (Dr. Jackson) told us that a year ago he had had under his care a man suffering from locomotor ataxy who could walk whether he looked on one side or at the ceiling. And he remarked that, although most patients suffering from ataxy say they "must look at their feet," some can walk without doing so if their eyes are open. The last-mentioned patient could walk with his eyes shut if he merely touched a wall with his fingers without resting any of his weight on it. Another patient, who was quite blind, and whom we saw with Dr. Jackson at the Hospital for Epilepsy and Paralysis, averred that he could walk for a long distance in the park if he just touched the railing with one hand; and, on trial, it was found that he could get along much better than we could have expected in this way. More than one patient has said that, when they shut their eyes, they "feel as if there was nothing under them." It seems only necessary in these cases, Dr. Jackson said, for the patients to establish some definite relation with the objects about them. The patient's wife said that he began to stumble about a year after the blindness. He had first pain in his back, and there were some lumps on it; but no precise information could be obtained on these points.

The patient has had difficulty with his bladder eleven years; and this may possibly be essentially a motor defect. If so, it is the only paralysis. The patient can stamp well with his legs, can resist flexion and extension fairly, and his legs are well nourished. There is no paralysis of any one of the cranial motor nerves, nor any history of squinting.

The man seems intelligent. His talk is rather odd, but there is none of the thickness of general paresis about it. He talks in the loud harsh way some people who have long been very deaf get into. He was so deaf that it required much patience to obtain a few facts from him. Dr. Jackson at first tried to communicate with him by writing words of monstrous size on paper. In this way, he got to know that the patient denied sexual excesses, and that he used to drink hard, and also, a fact that is worth notice, that he had smoked an ounce of tobacco a day for eighteen years.—*Lancet*, *March* 31, 1866, *p*. 346.

19.—ON SPASM.

By Dr. C. Handfield Jones, F.R.S., Physician to St. Mary's Hospital, and Lecturer on Medicine.

[By spasm the writer means that kind of muscular contraction which we observe in tetanus, and other similar affections.]

A point which seems to me very worthy of notice is, the affinity which is not obscurely marked between spasmodic contraction and paralysis, however widely at first sight the two states appear to contrast with each other. I will adduce some instances. Exposure to cold is a recognised cause of tetanus, and we have seen that it is also of facial and some other forms of paralysis. Dental irritation will sometimes cause trismus (a spasm), sometimes ptosis (a paralysis). Mr. Moodeen Sherriff has recorded some instances of paralysis not depending on organic lesion, in which there was rigidity of one limb, and simple immobility of the other on the same side, the same cause having given rise to both conditions, and the same treatment removing them. Dr. Little, many years ago, expressed the opinion, as the result of extensive observation, that with regard to spasm and paralysis the nature of the primary lesion in the central organ is the same in either case. He regards, however, paralysis as the result of a higher degree of lesion than spasm. A patient of mine who suffered from brachial neuralgia, which yielded to quinine and iron, stated that if she worked hard in the day she had not only severe pain at night, but also complete stiffness of the hand, induced, of course, by spasm of the muscles of the forearm. If she worked but little, the spasm did not occur. In this instance the influence of exhaustion in inducing muscular spasm is very apparent, as also the relation of the latter to the paralytic sensory disorder. A case of cataleptic affection is related by Mr. Buchanan, in which the muscles were rigid as iron during the attacks, which lasted twenty-four hours. They were induced by excitement, want of food, and fatigue, and subsided with the aid of rest, warmth, and an opiate. In most persons languor and weariness would have been the result of the above-named exciting causes rather than spasm. The inference, however, seems just that these states, dissimilar as their outward show is, are not in their origin essentially opposites. Mr. Bryant relates the case of a female, aged 17, who was suddenly attacked with contraction of the flexor muscles of the left hand and forearm a week before she came under his care. Her health was good, catamenia regular; the sensation of the part affected, perfect. Iron was given, and in three weeks the natural movements of the part were restored. The nerves of sensation, however, now became involved, and complete anæsthesia of the whole hand and forearm, made its appearance. This condition also disappeared in two months under the same treatment, sensation and movement being at last perfect. Here it can hardly be questioned that the spasm was the equivalent of the sensory paralysis. In a boy who was undergoing the operation of resection of the knee-joint, and had been rendered completely anæsthetic by chloroform, I observed the limbs in a state of marked rigidity, the arms more so than the legs. Here sensory paralysis coincided actually with spasm. A case is recorded by Plagge where a strong healthy soldier, aged 25, after eight hours' march, when the temperature was 63°, fell down suddenly, and lay pulseless, pale and without consciousness, with half-flexed limbs and closed jaws. Stimulants were given, and consciousness soon returned, and the rigidity ceased; but the patient could not move, and complained of severe pain in the back. Examination of the spinal column brought on immediately the most violent tonic contractions, without trismus or loss of consciousness, accompanied by the most frightful pains. An attempt to put the patient into a conveyance brought on the paroxysm afresh. Twenty ounces of blood were taken and morphia administered, whereupon the spine ceased to be painful. After a short journey the spasmodic contractions returned, but yielded to cold applications to the loins and stimulation of the extremities. The next morning the patient was quite well. Plagge entitles this a case of hyperæmia of the cord, but it seems to me to have much more claim to be regarded as one of exhaustion with consecutive nervous disorder. Had active hyperæmia of the cord really existed, recovery could not have ensued so rapidly. The primary sudden syncope, the severe pains, and spasms, are much more consonant with the view of a tetanoid than of an inflammatory affection. It is most probable that the above was an instance of the malady termed by Trousseau tetanilla, which is induced by diarrhœa, fevers, lactation, and cold, and is treated advantageously by venesection and sedatives or quinine. In the description given of this disorder, it is stated that the intermittent contractures are usually preceded and accompanied by loss of muscular power; the hands, though half flexed, cannot be completely closed, and the tactile sensibility is also much impaired,—in fact, both in the causes and phenomena of this neurosis there is an evident affinity indicated between spasm and paralysis. I remember when taking my first lesson in aquatics, with youthful enthusiasm, that my fingers for some time afterwards were contracted, and retained the position they had taken while grasping the oar—a contrary result to what one would have expected from the prolonged fatigue of the flexor muscles. A friend tells me he experienced the same, and so I suppose did the Israelitish worthy who arose and smote the Philistines until his hand was weary and *clave* unto his sword. In a case recorded by Mitchell severe spasms of the facial muscles, cervical and lingual, were found to depend on the presence of carious teeth. This cause would most commonly have given rise to neuralgia, which, as we have just seen, has decided affinities with paralysis. If Earle records a case in which trismus was cured immediately by the discharge of a tapeworm, a case I have cited in Lecture II. proves that the same cause may give rise to facial paralysis. If we pass from tonic spasm to clonic, we find still more frequent indications of its affinity to paralysis. It may be sufficient merely to allude to choreic affections, which often contain quite as much of one morbid element as of the other. The same is exemplified in disorders of the internal organs depending on debility. I met a medical friend some years ago in the street, who was looking pale. Said he to me, "My solar plexus is worn out; I want a new one. I am not ill otherwise; but I can't keep anything on my stomach. I have not been out of London more than a day consecutively for six years." Here was surely a state essentially depending on nervous paresis, but presenting phenomena of irregular muscular action. Such cases are sometimes cured by strychnia—a fact which further illustrates their true nature.

The preceding observations appear to me to afford considerable evidence that spasmodic contraction is a very different thing *quoad* innervation from voluntary, and is a morbid mode of action related to paralysis, much in the same way as neuralgia is to anæsthesia. It is, of course, impossible to state in what respect the molecular condition of a nerve-cell which gives rise to a spasm differs from that which gives rise to paralysis. The constituents of the living tissues are not accessible to our microscopes; and, if they were, it is by no means certain that we should be able to distinguish any visible deviation from their healthy appearances in conditions of mere functional derangement. It is conceivable that some such changes in the nervous currents as occur during polarisation in luminous undulations may be the cause of neuralgia and spasm in the sensory and motor nerves respectively. I am utterly disinclined to enter into barren speculation, but it seems to me by no means such to endeavour to form a just estimate of the *nature* of the morbid actions which come before us, and to ascertain in what direction they differ from the healthy. We may be quite unable to acquire a full and complete knowledge of a disease, but we may, nevertheless, obtain very practical and useful knowledge respecting it. Thus we may never find out precisely what malaria is, or be able to demonstrate exactly in what way it acts upon the frame; but it is a highly practical and important point to know that it enfeebles and lowers nervous energy, and that most of its morbid effects can be explained on this view of its operation.

It seems to me an established point that the *quality* of nervous action differs greatly in different instances of disease, and that we cannot regard it as either simply increased or diminished in energy. This does not seem to me to have been always kept in view. In the motor nervous apparatus we have the state of healthy vigorous action replaced by tonic or clonic spasm, by tremors, choreic jerkings, and finally by paralysis, all without roganic lesion. Muscular contractions, which our common persuasion holds, and I think correctly, to be an evidence of existing nervous power called into play, are most evidently not in all cases produced by the same *kind* of nerve action. Many actions might indeed be classed as *actions of weakness*, in apposition to actions of *strength*. The depression of the jaw in yawning when we are weary and languid is surely, *quoad* nervous power, not the same kind of action as that which occurs when an hungry man is eating, and the jactitations of chorea are not prompted by the same kind of nervous impulses which direct the strong, steady strokes of an oarsman rowing a wager. The movements of chorea are usually readily controlled by a steady grasp of the agitated limb, but I had recently under my care a girl whose arm I was quite unable to keep still by all the force I could exert with both my hands. At the same time the legs were weak and tottery. Here the morbid condition of the lower part of the cord must evidently have been different to that of the upper. In some epileptic attacks there is much more tetanic rigidity than in others, yet we cannot think that different nervous centres are affected in the one case and in the other. In the sensory apparatus we have gradations from healthy sensation by hyperæsthesia, endless varieties of dyæsthesia and neuralgia, down to complete anæsthesia.

Without recognising the different quality of nervous action in different instances, it would be impossible to explain the great varieties we find in the action of certain medicines. This remark leads us again to consider whether, while we view spasm as the result of a particular kind of action in nerve cells, there may not be varieties of this special mode of action itself, some being of a more sthenic, others of a more asthenic character. Certain cases of tetanus are beneficially affected by aconite, and show a very remarkable power of resistance to this potent agent; others are not at all relieved by it. The same may be said of nicotine. Conway records two cases of traumatic tetanus cured by the exhibition of tartar emetic in large doses, while in other instances free stimulation and tonics appear to have been successful. The spasm of asthma is sometimes markedly relieved by depressants, as emetics and tobacco, sometimes by free doses of alcohol and ether. Some cases of chorea are much benefited by antimony, some by sedatives, some by tonics. The above-mentioned facts seem to me to indicate plainly that all spasmodic actions are not of the same quality, and that the same sort of difference prevails among them as there does between instances of hyperæsthesia and neuralgia. To recognise this is, I think, important, because it does away at once with any idea that the disorders of which spasm is the characteristic element are to be treated in any unvarying manner, even when we have arranged them in their classic groups. Nothing impresses me more as I grow older in practice than the manifoldness of disease, and how insatiable is the variety which Nature exhibits in her deranged as well as in her healthy aspects. We are too prone to imagine that where we see an outward show of resemblance there exists an internal and real identity; and this idea is hard to displace, though it be negatived again and again by the testing action of remedies. I will now attempt to apply the above views respecting neuralgia and spasm to a disease respecting the true nature of which authorities are by no means agreed—I mean angina pectoris. Dr. Stokes says, "If we inquire into the nature of a paroxysm of angina, two completely opposite views present themselves. Is it to be explained by spasm of the heart or referred to diminished action?" Now, the position I would take is that these views are not completely opposite; that spasm of the heart is much more akin to paralysis of the organ than to healthy contraction. The latter is unattended by pain, or indeed by any feeling except it be a certain indefinite sense of "wohlsein," and what we may term whole-heartedness. The former is essentially and distressingly painful, and attended with that sensation of imminent peril of life which is characteristic of all, even of minor, cardiac dysæsthesia. The pain I regard as the more constant and essential phenomenon, and the muscular spasm as a secondary and varying. It is of course possible that death might result during an attack from closure of the cavities in tetanic contraction; but though Lussana is inclined to adopt this view, the evidence in favour of it seems to me but scanty. There are only three observations in which the heart is stated to have been decidedly contracted after death, and it is reasonable to conclude that in many of the cases where the muscular tissue has been found more or less diseased the condition of the cavities was very different. But even could it be shown that the fact was otherwise it would be of no great weight, for it seems most probable that the post-mortem condition of the heart is determined in this disease, as we saw that it was in peritonitis and in similar states, by the amount of contractile energy possessed by the cardiac muscle. If this is great, the cavities close in a kind of rigor mortis, the contractility of the fibre outliving the inhibitory influence of the neuralgic pain; if it is the reverse, as one may conclude where the organ is found thinned, softened, or fattily degenerated, the cavities will remain more or less open. The treatment found most beneficial in angina is evidently such as contemplates a stimulating and invigorating rather than a sedative effect. Opium is, indeed, to be given in tolerably full doses, but this is for the purpose of counteracting and quelling the pain, and not of producing any narcosis of the nervous system. Stokes seems to be averse from employing this remedy when the heart's action is very weak. Stimulants are by general consent accounted our chief remedies in angina, and I am free to confess that I hold with those who look on opium as primarily and essentially (at least, in moderate doses) a powerful though peculiar nervine stimulant. The most practical point for consideration in all cases of angina is whether the breast-pang which threatens to, and sometimes actually does, arrest the heart's action is of organic or non-organic origin. If we are obliged to consider that the former is the case—that the neuralgia has its starting point in a nervous filament pressed upon by a patch of calcareous deposit in the coats of the aorta or coronary arteries—the prospect is of course, very unfavourable. If, on the other hand, we can hope that some existing rheumatic, gouty, or malarious disorder has produced the neuralgia, we may be more hopeful, though I have known a patient die in whom an autopsy demonstrated the absence of any organic lesion.

I will now enumerate the principal conceptions which I have endeavoured to lay before you in these lectures.

(1) That of the direct paresis of a nerve or nervous centre as the result of the action of some imponderable influence.

(2) That of inhibitory paresis where the result is produced by some morbid irritation, the vasomotor nerve centres seem to be more liable than others to suffer in both these ways.

(3) That of the influence of nerve-prostration in altering the texture of capillary vessels at the same time that it dilates arteries, thus giving rise to extravasation of blood in greater or less quantity.

(4) That of the paralytic character of neuralgia and hyperæsthesia.

(5) That of the morbid affections of the nervous centres being very analogous to those of the nerves.

(6) That of the affinity between spasm and paralysis.

I am well aware that the grounds which I have assigned for the views above enunciated are insufficient to demonstrate their truth, and when I compare my own work in these matters with the long laborious researches of others, I feel very conscious of short-coming. Still, it is to be remembered that when we come to stand at the bedside, we often feel the want of more than the bare facts which we can ascertain by observation ; we need to have also ideas appropriate to the facts, and which may enable us to interpret their meaning aright. I am not without hope that these lectures may have done something to supply such conceptions, or to make them more familiar, and if this be the case I shall be well content.—*Med. Times and Gazette, Feb.* 17, 1866, *p.* 167.

20.—ON THE CAUSES, DIAGNOSIS, AND TREATMENT OF FUNCTIONAL NERVOUS AFFECTIONS.

By Dr. C. E. Brown-Sequard, F.R.S., &c.

One of the most important of the recent advances in physiology and pathology consists in the demonstration that the nervous conductors of the various kinds of sensitive impressions and of the reflex phenomena, and also for the transmission of nervous force to muscles, blood-vessels, &c., are absolutely distinct one from the others as regards their functions. Leaving aside the nerve-fibres of the brain, some of which have functions altogether different from those of other parts of the nervous system, I have ascertained that, besides the four distinct kinds of nerve-fibres of the higher senses, there are at least eleven distinct kinds of nerve-fibres in the spinal cord and in the cranial and other nerves. The following table shows what are the distinct functions of these eleven kinds of nerve-fibres. They are—

1st. Conductors of impressions of touch.
2nd. ,, ,, tickling.
3rd. ,, ,, pain.
4th. ,, ,, temperature.
5th. ,, ,, contraction (muscular sense).
6th. Incito-motor conductors.
7th. Incito-nutritive and secretory conductors.
8th. Voluntary motor conductors.
9th. Involuntary motor conductors.
10th. Vaso-motor conductors.
11th. Nutritive and secretory conductors.

I hardly need to say that the number of distinct nerve-fibres is probably much greater than is shown by this table ; but the demonstration of the distinction of other kinds of nerve-fibres (such as those serving to sensations of hunger, thirst, pressure, or to voluptuous sensations, &c.) is not yet sufficient.

Almost all the symptoms of functional (and I might say, also, of organic) nervous affections take place through one or other of three modes of alteration of the properties and functions of the fifteen kinds of conductors above-named. Those three modes consist in: 1st, a diminution or loss of power; 2nd, an increase of power; 3rd, a morbid state producing a great variety of phenomena. In the nine kinds of conductors serving for the transmission of sensitive impressions the first of these three modes of alteration constitutes *anæsthesia*, the second *hyperæsthesia*, and the third is the cause of *morbid sensations* (including the so-called *referred sensations*).

I will not enter into details here about the kinds of nerve-fibres which I call incito-motor, incito-nutritive or secretory, or about those which I simply name nutritive or secretory; but as it would be impossible to understand the future lectures of this course without some knowledge of the properties and functions of these nerves, I will give, as briefly as possible, a few notions concerning their physiological and pathological history.

1st. *Incito-motor nerves.*—These are the well-known excito-motor nerves of Marshall Hall. A great many facts prove that they are absolutely distinct from the sensitive nerves. All the reflex movements of the muscles of organic or animal life (including the excretory ducts of glands, the blood-vessels, &c.) originate through an irritation transmitted by these incito-motor nerves.

2ndly. *Nutritive and secretory nerves, and incito-nutritive and secretory nerves.*—That more appropriate names should be given to these nerves, I will not deny; but so long as their real mode of action remains somewhat doubtful, I think it is better to call them by names which only imply what we positively know of them—i.e., that they are agents of modification of secretion and nutrition. The *secretory and nutritive nerves* are in many respects the antagonists of the *vaso-motor nerves.* While these last nerves, when put in action, produce a contraction of blood-vessels and all the phenomena which I have shown to ensue from that contraction (such as a diminution in the quantity of blood, a diminution of sensibility and other vital properties and of secretions), the nutritive and secretory nerves, on the contrary, when put in action, occasion a dilatation of blood-vessels and all the phenomena that ensue from it, as shown first by experiments of Czermak and of Cl. Bernard. I proposed long ago (in my lectures at the College of Surgeons in 1858) an explanation of the action of these nerves, which becomes more and more probable with the frequent discovery of new facts concerning these nerves. The explanation is, that the secretory and nutritive nerve-fibres act upon the tissues so as to increase their chemical interchanges with the blood, and that, in consequence of this increase of chemical interchange, blood is more attracted to the part, and, as an effect of this greater afflux of blood, there is a dilatation of blood-vessels. In many of the future lectures I will mention phenomena due to this dilatation of blood-vessels from an irritation of this peculiar kind of nerves. I will only add now to what I have already said on this subject, that it is chiefly by a reflex action that these nerves produce their peculiar effects. The redness, the congestion of parts attacked with neuralgia or of neighbouring parts, the secretion of tears when the eye is irritated or the skin of the face pinched, the secretion of mucus from the nose, or from laryngeal, trachial, or bronchial mucous membranes, or the diarrhœa produced by cold, and an immense number of facts in which there is a congestion or a secretion as a result of an irritation of cutaneous or other nerve fibres, are all effects of the peculiar influence exerted by secretory and nutritive nerve-fibres on glands and other tissues.

These nutritive and secretory nerve-fibres are, as I will try to prove, in future lectures, the agents of production of inflammation, suppuration, and ulceration, by a reflex action. They are the channels of production of meningitis, of encephalitis or myelitis, when those inflammations are due to cold, to a burn, or to a visceral affection. They also have a great share in the production of many functional nervous affections, and particularly tetanus. Besides, through the agency of these nerves much may be done for the treatment of nervous affections, as I will show hereafter.

The incito-secretory and nutritive nerve-fibres are the incident or centripetal nerve-fibres which, by a reflex influence, act upon the centrifugal secretory and nutritive nerve-fibres. Their distinct existence is not fully demonstrated, but many facts render it extremely probable.

The causes of functional nervous affections can be divided into several groups. 1st. An irritation by worms, by teething or decayed teeth, by cold, by a burn, a wound, an inflammation, a neuralgia, &c., of centripetal nerve-fibres (the incito-motor, the incito-nutritive, and some others). 2nd. An alteration in the quantity or the quality of blood. 3rd. Both the two preceding kinds of causes coexisting together, as in cases of typhus fever, of variola, of diphtheria, of uræmia from disease of the kidneys, &c.

I will not now insist on the mode of action of these causes, as that will be better done in treating of each nervous affection separately; but there are a few general remarks which will be in their proper place in this introductory lecture. There are two general rules which particularly deserve attention :—

1st. *The same peripheric cause of irritation acting on the same centripetal nerve may produce the greatest variety of effects, including every functional nervous affection or disorder.* This is well illustrated by the effects on the eye of a neuralgia of the infra- or supra-orbital nerves. There are cases showing: 1st, spasm of the sphincter of the pupil; 2nd, mydriasis; 3rd, spasm of the orbicularis palpebræ; 4th, paralysis of the orbicularis; 5th, paralysis or spasm of one or several of the muscles of the ocular globe; 6th, photophobia; 7th, amblyopia or amaurosis; 8th, congestion or inflammation of the conjunctiva or of other parts of the eyes; 9th, diminution or increase of the secretions of the lachrymal and other glands; 10th, a cataract; 11th, a glaucoma.

Other instances, though less striking, might be adduced. I will only mention what we know as regards the effects of cold air on persons coming out of a theatre. One may be attacked with a sore throat, a second with ophthalmia, a third with enteritis, a fourth with nephritis, and many others with any other visceral inflammation; while one may be attacked with a facial paralysis, and others with almost every other partial paralysis, or with chorea, with contracture, with meningitis, or an inflammation of some part of the nervous centres. It is true that here it is not the same part of the skin which is acted upon by the sudden lowering of temperature; in most of such cases, however, it is the front part of the chest and the neck that are subjected to the influence of cold air.

It is probable that the great variety of phenomena following an irritation of centripetal nerve-fibres depends on differences of excitability of some parts of the nervous centres in different individuals. I have examined carefully, on a large number of men, the effects of tickling the sole of the foot, and found that these phenomena differed considerably in different individuals. In one, laughter predominated; in another, involuntary screaming, or shedding of tears, or jerks either in the irritated limb or in both lower limbs, or a general trembling, or a spasm of the diaphragm, or an almost tetanic rigidity of the irritated limb. I need not add that in some individuals there was hardly any effect produced when, being prepared for the irritation, they fought against its influence—a fact which, with many others, shows that by an effort of the will we sometimes can, at least in a certain measure, prevent the production of reflex phenomena.

I will not say more at present on this subject, leaving for other lectures the demonstration that it is through a real reflex action (i.e., through a mechanism similar to that of reflex movements) that the morbid effects of an irritation of centripetal nerve-fibres take place.

2nd. *The degree of excitability of the different parts of the nervous system may increase or decrease considerably in the same person under the influence of various causes.*—This proposition is of so great an importance in the diagnosis and treatment of nervous affections, that it is only by the light it throws on many otherwise obscure cases that we are enabled to recognise their nature, and to apply the proper treatment. Many parts of the nervous system that are completely, or almost completely, inexcitable in a healthy condition, become excitable, and sometimes in a wonderful degree, under the influence of several morbid causes. Amongst these causes, I will point out the grey matter of the spinal cord, and the nerves of the tendons, aponeuroses, dura mater, periosteum, bowels, bladder, kidneys, and some other viscera. Many causes, very different one from the others, may increase the excitability of the various parts of the nervous system. I will mention here only the principal of these causes: 1st. I have found that muscles, nerves, and the spinal cord become more excitable when laid bare, and particularly when the air in contact with them is richer in oxygen than the ordinary atmospheric air. 2nd. A congestion or an inflammation will increase the excitability of nervous tissues everywhere, but nowhere so markedly as in the grey matter and some parts of the white columns of the spinal cord, which, in consequence of that increase, will become able to produce referred sensations of pain, of cold or heat, of tickling, &c., and phenomena due to an irritation of motor, vaso-motor, and nutritive nerves. 3rd. An afflux of blood, such as occurs merely by gravitation, or after a a section or a paralysis of the sympathetic nerve, or a lesion of the spinal cord, the medulla oblongata, or the base of the brain, will also increase the excitability of peripheric nerves in the parts where this afflux takes place. 4th. Certain remedies or poisons (stychnine particularly) will increase the reflex excitability of the spinal cord to a wonderful degree; while others, such as atropine, chloroform, &c., will diminish it considerably. 5th. Certain diseases, such as tetanus and hydrophobia, will increase extremely the reflex excitability of some parts of the cerebro-spinal axis. 6th. A great loss of blood, anæmia, chlorosis, will also increase the reflex excitability of the nervous centres.

The increase of reflex excitability in cases of extreme debility, as in old age, or after a loss of blood, or other causes of insufficient nutrition, would be very difficult to understand if we did not know that the reflex excitability of the spinal cord can be increased under the influence of certain substances (strychnine, principally) when no blood at all remains in the blood-vessels of that nervous centre, as proved beyond the possibility of doubt by Messrs. Martin-Magron and Buisson. The excitability of sensitive nerves may also be increased when the quantity of blood is much diminished, as we often observe in fingers that have been exposed to cold air or cold water. It seems, from a review of all the facts I know bearing on this point, that certain substances contained in blood altered in quantity or quality will act on the excitability of nerve-fibres in the nervous centres, or in the nervous trunks and branches, so as either to increase it, as is done by strychnine (on the grey matter) and oxygen (everywhere), or to decrease it, as is done by carbonic acid, atropine, chloroform, &c.

To conclude what I wish now to say on the excitability of the nerves, I will only mention—1st, that I have ascertained that the excitability of the same nerve varies in different parts of its length, and to such a degree that in some parts the excitability seems nil or is very slight, while in other parts it is considerable; 2nd, that I have shown by positive experiments that the excitability of muscles, of nerves, and of the spinal cord may be very much increased while the *force* developed by the action of those parts is very small. For instance, atrophied muscles unable to contract with half the force shown by healthy muscles, will, however, contract under the influence of an excitation that will produce no effect on healthy muscles.—*Lancet, Jan.* 6, 1866, *p.* 1.

21.—ON THE MORAL TREATMENT OF FUNCTIONAL NERVOUS DISEASES.

By Dr. C. E. Brown Sequard, F.R.S., &c.

[In the treatment of functional nervous diseases it is of the utmost importance that the patient should have some serious aim before her—to prevent or to check nervous disturbances.]

The applications of this principle are, of course, very difficult and often impossible in certain neuroses; but in those cases in which any kind of serious work, not too fatiguing or exciting, is liked by the patient, she should be induced to do it. In cases of hypochondria, of hysteria, of chorea, and even of epilepsy, a great benefit can be derived from a serious employment of the mental and physical activity of the sufferer. How often have I not seen young epileptics kept in idleness (alas! by medical advice), and, having gained more or less of the vices it leads to, improve rapidly from having their minds occupied at regular hours, in the same way as healthy people of their age. The second principle of which I will speak now is that we must, in the interest of our nervous patients much more than in our own, give them confidence and hope in the treatment we recommend. In hysterical and all nervous complaints allied with it, and also in hypochondria, and in several other neuroses, a great hope of cure will do much to work out the cure. No doubt you will say, How to give hope? I answer that the best means for that purpose is to have hope ourselves, and to express our hope with the accent of conviction. And as you would ask, How can we command hope in ourselves? I answer that the very knowledge of the truth of the principle I am now speaking of is enough to render one hopeful. I need not repeat that I am now speaking only of those neuroses in which the power of the mind upon the body is immense, and so much so that in some forms of these neuroses a sudden or almost sudden cure is not very rare.—*Lancet, Jan.* 27, 1866, *p.* 87.

22.—ON THE USE OF BROMIDE OF AMMONIUM IN THE SLEEPLESSNESS OF FUNCTIONAL NERVOUS DISEASES.

By Dr. C. E. Brown Sequard, F.R.S., &c.

It is of the utmost importance to improve the sleep, which is generally so bad in patients attacked with a morbid increase of the reflex excitability. For this purpose an invaluable remedy has recently been discovered: it is the bromide of potassium. Excepting when pain is one of the causes preventing sleep (in which case opium, aconite, and belladonna should be employed together), I have found that this remedy has a most wonderful power to produce a quiet and refreshing sleep, without any drawback that I am aware of. I usually give to adults a dose of thirty grains of that salt a quarter of an hour before the last meal, and a second dose of from thirty to fifty grains at bedtime. In cases in which, without any nervous complaint, there is sleeplessness owing to some cause of cerebral excitement, as well as in all neuroses, excepting hydrophobia, tetanus, very severe cases of delirium tremens, and some forms of insanity, sleep is almost always induced by that remedy. In some cases I have found it necessary to increase the dose of the bromide, and to give also one grain or one grain and a half of codeine an hour before bedtime. In those affections in which the bromide of potassium is not powerful enough as a sleep-inducing agent, a warm bath of four, five, or six hours' duration is often successful.—*Lancet, Jan.* 27, 1866, *p.* 87.

23.—CASES ILLUSTRATING THE USE OF BELLADONNA AND BROMIDE OF POTASSIUM IN EPILEPSY.

Under the care of Dr. Broadbent, St. Mary's Hospital.

Dr. Broadbent has for some time been engaged in observing the effects of two remedies—belladonna and bromide of potassium—in epilepsy, in cases where no positive indication for treatment has existed in deranged function of abdominal or other viscera. The belladonna is given in the form of extract in gradually increasing doses till physiological effects—dilatation of the pupil, with dryness and redness of the fauces, &c.—

become apparent. The quantity is then slightly diminished, so as to allow the unpleasant symptoms, such as the sense of constriction of the throat, to pass away; but the pupil is kept dilated for about a month, when the administration is suspended for a short time, to be again resumed. No precise rule has been followed with the bromide. It has generally been given in some bitter infusion, with compound spirit of ammonia.

A few of the cases which have been sufficiently long under observation to furnish results are given, classified according to their probable causation, malformation, or organic disease of the brain, injury to the head, sexual irregularities, &c.

Organic affections of the brain.—E. T., an idiot boy, aged 11 years, partially hemiplegic on the left side, the limbs not contracted, the arm hanging, the leg dragging, had, when brought to the hospital on the 6th of April, 1865, three or four fits every day, and six or seven in the night. His mother had paralysis of the left facial nerve, which came on twelve months before he was born. She had a fall also about a fortnight before she was delivered of him.

The boy was ordered one-third of a grain of extract of belladonna three times, and then four times, a day. On the 20th of April, half a grain, with the same directions. Considerable improvement followed; and in the week ending May 18th he had only three fits. On the 25th, however, he had been poorly all the week, did not walk so well, and had had fits every night. The pupils were greatly dilated. It was not considered prudent to go on with the belladonna, and the bromide of potassium was given in doses of six grains, increased on June 15th to eight grains. The fits at once diminished in frequency, and he improved in every way till June 13th, when he was worse than ever. The bromide was suspended for a time, and again resumed; but in October, when he ceased to attend, there was no marked improvement, though the fits were not so numerous as when he first began to attend.

A. H., when first seen Sept. 16th, 1863, was four and a quarter years old. He had nine or ten fits every day, the first having occurred when he was a year and a half old, and in it the left arm became paralysed and contracted, and had remained in this condition. He was well nourished. The head was large and remarkably long; narrow, projecting, and almost pointed posteriorly; flattened at the top; concave at the situation of the anterior fontanelle. He had an intelligent look, and was said to be talkative.

Belladonna was given in gradually increasing doses, and the fits slowly diminished in number till, on Nov. 25th, he was said to be better than at any time for two years. On Dec. 4th numerous fits occurred, and he was insensible for two days afterwards. There was now loss of power in the right arm as well as in the left; the arm could be moved, but it jumped. The belladonna was continued. The fits averaged seven a week till the 23rd, when he had seven, and on the 26th twenty-five. On January 20th, 1864, he had only had two fits in twelve days; then he was in fits for an entire night.

These alternations continued—the intermissions being at times so long as to give encouragement in the use of belladonna—till July 11th, when the bromide was given, at first in doses of three grains, and it seemed to have some good influence. The quantity was raised successively to five, six, and eight grains; and from the beginning of Sept. 1864 to the end of Jan. 1865 he had only occasional slight attacks, the weekly reports being "no fits," or one, two, or three (once seven), but "not to go off." He could also open the left hand, which had hitherto been firmly clenched, and seemed better in every respect. On Feb. 3rd, however, and on the 16th and 17th, he had numerous fits. The dose of the bromide was increased on the 3rd to ten grains. On March 31st he had again severe fits, which affected his speech; also on April 12th, after which he was deaf for a time. Early in May the remedy was suspended for ten days, but, the fits returning with great severity, it was resumed. He again became better, and ceased to be under Dr. Broadbent's care in June.—*Lancet, Jan.* 27, 1866, *p.* 92.

24.—ON THE TREATMENT OF EPILEPSY.

By Dr. C. E. Brown Sequard, F.R.S., &c.

[Probably the principal mode of action of the following remedies is through the irritation of the excito-motor nerves of blood-vessels, and excito-nutritive and secretory nerves.]

1st. *Ligatures, pinching, rubbing, and circular blisters.*—When I first began to make use of ligatures in epilepsy I had hardly any other than theoretical objections against the view that in that convulsive affection ligatures serve only to prevent the passage upwards of an irritation travelling from the periphery towards the nervous centres, I soon saw several cases similar to a few already known, in which the cause of the aura was clearly in the nervous centres, and in which, however, ligatures acted as marvellously in preventing attacks as in cases in which an aura had undoubtedly its cause in a peripheric part. When the extremely various sensations which accompany an aura, like the aura itself, have their only cause in the nervous centres, as it was in the cases I have just mentioned, it is quite evident that there is nothing going from the periphery to the nervous centres, and as evident that ligatures in such cases must act by some other means than by preventing the passage of something. This first point being clearly established, I found that ligatures, in cases of an aura from a centric origin, acted with greater efficacy when they suddenly induced powerful irritation of the nerves of the skin than when they were applied gradually and slowly, either tightly or not. After having made this step, I ascertained that it was so even in cases of an aura starting distinctly from the periphery. I found afterwards that in both kinds of cases, pinching, pricking, striking, or rubbing the part which is objectively or only subjectively the place of origin of an aura may prevent a fit of epilepsy or of hysteria, or simply of local or general convulsions. I was led therefore to look upon ligatures in those cases as acting chiefly as a means of irritation of the nerves of the parts on which they are applied, irritation producing a favourable change in the nervous centres. The following results I have subsequently obtained:—1st. That it is not necessary to apply an irritation (by a ligature, pinching, &c.) on the very limb from which an aura seems to start, as the same means applied elsewhere may succeed: but the chance of success is much greater by the former than by the latter way. 2nd. That a constant or a frequent irritation (by a blister, an issue, a seton, the actual cautery, &c.) on the place from which an aura seems to start may not only prevent fits, but, by some change of nutrition locally (if the aura is really of peripheric origin) and in the nervous centres, may reduce or even destroy altogether the tendency to fits, and lead to a complete cure. 3rd. That, as a circular ligature may procure a temporary good effect, so a narrow *circular blister* applied all round a limb, a toe, or a finger, or a circular cauterization with a white-hot iron, may cure epilepsy, hysteria, neuralgia, &c., in cases with a distinct aura. 4th. That even in cases in which there is no known aura, felt or unfelt, ligatures, pinching, and other means of irritation may prevent fits when applied on persons expecting a fit. Before I pass to another subject I ought to say that for a long while a positive fact has been known—not generally, however—which shows that ligatures applied to the limbs when a fit of fever and ague is about to take place may prevent the fit. As malarial poison produces fever by a peculiar influence upon the spinal cord, so the success of ligatures against fever and ague is due to a favourable change in a nervous centre, induced by a peripheric irritation of incident nerves.

2nd. *Cold.*—Ice is the most powerful means we possess to produce a reflex contraction of bloodvessels. Before a positive demonstration was obtained on this point, it seemed already extremely probable from the following well-known facts: that ice applied on the abdomen or on the back, or in the vagina and rectum, often stops a hemorrhage from the womb: that, applied to the head, it will do some good in cases of congestion or inflammation of the brain or its meninges, or in cases of symptomatic or idiopathic headache; that it may stop a hæmoptysis when applied to the chest—a hemorrhage from the bowels, the stomach, or the kidneys, when applied to the abdomen or the loins; that it may be employed with marked benefit against an inflammation of the viscera or serous membranes of the chest and abdomen, and especially against metroperitonitis, as shown particularly by Kiwisch and Béhier; that, applied all along the spine, it may cure tetanus, and be useful against spinal meningitis. These facts gave a great probability to the view that ice acts as an irritant of incident nerves, and through the influence of this irritation on the spinal cord or base of the brain produces a reflex contraction of bloodvessels; but, so far as I know, there was no positive fact establishing this peculiar influence on bloodvessels, until the experiments I made about fifteen years ago with my friend Dr. Tholozan clearly showed this important influence of ice. We found that dipping one hand into water at feezing-point produces in the other hand considerable contraction of bloodvessels and a corresponding diminution of temperature. This fact, of which I shall have to speak again in another lecture, leaves hardly a doubt as regards the mode of action of ice in the cases in which it is daily employed with the view of producing a remote effect. Recently, Dr. Chapman has had the idea of applying ice to the spine, with the purpose of paralysing the vaso-motor nerves of the limbs or of the head. He states that the application of ice to the lumbar region of the spine produces a dilatation of the bloodvessels of the lower extremities, and all the consequences of a greater afflux of blood; and that a similar application between the shoulders produces the effects of the section of the two cervical sympathetic nerves. Whether he be right or wrong in his explanations, he at any rate relates facts which deserve to attract the attention of the profession. We are not frequently enough successful in our treatment of neuroses to neglect means which, according to his statements, have cured so many cases of epilepsy or of other nervous affections. It is to be hoped, therefore, that physicians of large hospitals will give a fair trial to the means he proposes, and ascertain the real value of cold applications to the spine in the treatment of neuroses. Before dismissing this subject, I will mention that a patient of Mr. Ernest Hart has been cured of epilepsy and progressive atrophy of the optic nerve by his applying ice to the spine, and that the same patient had only derived a slight benefit from the use of the valerianate of atropia under my care. This would be the place to speak of hydrotherapy, if I had time to treat of all the therapeutical means. I will only say that an important addition to the proofs of the reflex influence of cold water on bloodvessels has been obtained by Dr. L. Fleury, who by careful measures, has ascertained that, after the application of a cold douche on the skin, a congested spleen or a congested liver will sometimes diminish in volume much more than had been stated by other practitioners. Several interesting facts lately observed by a talented young physician, Dr. Beni-Barde, give a confirmation to the view that the therapeutic influence of hydrotherapy is due to a reflex action on the internal organs from the excitation of the skin by cold water.

3rd. *Actual cautery and other applications of heat.*—Jobert de Lamballe, Vallieix, and others have exaggerated the advantages of the so-called transcurrent cauterization against neuralgia; but there is no doubt that this means is one of the most valuable in that affection, and is very useful against other neuroses. I may say that it is not at all necessary to produce a burn that will give a great discharge. If the iron employed has a smooth surface; if, besides, it is applied only at white heat, and passed quickly over the skin with a gentle pressure, it hardly gives pain. The epidermis and a very thin layer of the skin dry up and fall of within two or three days, leaving a reddish mark, which disappears before very long. This means I have employed with great advantage in a case of angina pectoris, and in many cases of epilepsy and neuralgia, and with some benefit in cases of contracture, of torticollis, of paralysis agitans, &c. The application of heat in many other ways (by Mayor's hammer, for instance) is very useful in a great many cases of nervous affections. In a case of coma with convulsions, due to a complete arrest of the urinary secretion after an attack of renal hæmaturia, in 1851, Dr. Tholozan and myself, after twenty-four hours of unsuccessful treatment, decided to apply heat on the skin of the loins and sides of the abdomen. A large silver spoon was dipped into boiling water, and was applied on eight or ten places. To our surprise and delight, we soon found the respiration improving, and in less than a quarter of an hour the convulsions ceased, and the patient came to his senses and passed a little water. The return of the urinary secretion had saved him. No doubt the irritation of the skin had acted by a reflex action on the secretory nerves of the kidneys, and produced the secretion of urine.

4th. *Alternative applications of cold and heat.*—Led by the well-known powerful influence of alternative immersions of newly-born children into cold and warm water in restoring respiration, I have often applied, with more or less good effect, cold and hot water to the spine, with a view of producing a contraction of bloodvessels of the spinal cord. Whether these vessels are already somewhat contracted, as is most likely the case in reflex paralysis or reflex anæsthesia, or dilated, as is often the case in hysterical paraplegia, some good may be obtained in these two opposite conditions from the use of cold and hot applications, owing to causes which I will explain in another lecture. I have found alternative applications of ice and hot poultices very useful against neuralgia. Dr. Ramskill has obtained some benefit from alternative immersions of a paralysed limb into very cold and hot baths.

5th. *Cauterization of fauces and larynx.*—A man of some talent, who unfortunately allied eccentricity to a genuine love of his profession, Dr. Ducros, used to treat many nervous and other complaints by the application of ammonia to the fauces. More rational researches have shown that a cauterization of the fauces, and still better, of the mucous membrane of the larynx, is of the utmost value in the treatment of whooping-cough, of several other spasmodic affections of the larynx and the bronchial tubes, of diphtheritic or hysterical paralysis of the larynx and pharynx, and in that form of epilepsy which is due to laryngismus.

6th. *Other kinds of irritation of sensitive or incident nerves.*—Many applications on the skin have a great power in changing the state of nutrition of the nervous centres or of the peripheric parts attacked with pain, spasm, &c. Of the means of treatment of this kind that have recently been proposed, I will only point out the use of nitrate of silver and of sulphuric acid on the skin in cases of neuralgia; the use of the continuous galvanic current, as proposed by Remak in many nervous affections, and the singular, but certainly sometimes successful application of sulphuric ether in the external ear against functional deafness.—*Lancet, Feb.* 10, 1866, *p.* 139.

25.—ON THE USE OF CHLOROFORM IN EPILEPSY AND HYSTERIA.

By Dr. C. E. Brown Sequard, F.R.S., &c.

[Besides being useful as a means of preventing an expected attack of hysteria or of epilepsy, inhalations of chloroform are useful in three particular kinds of cases.]

1st. When the comatose state due to uræmia or following a violent attack of epilepsy is frequently interrupted by tonic or clonic spasms of the respiratory muscles, increasing the profoundness and duration of the coma, I have employed inhalations of chloroform without any appearance of greater harm than is usual in surgical cases, and with the beneficial result of shortening the duration of the stupor by preventing the spasmodic contractions or diminishing their violence. This mode of treatment is known to be particularly useful in almost analogous circumstances—i.e., in cases of puerperal convulsions. In many cases of epilepsy in which a large number of attacks usually occurred in a day or a day and a half (such a series of seizures being followed by complete freedom from fits for three, four, or six weeks), and in which also a comatose state usually appeared soon after a few attacks had taken place, I have found the benefit due to chloroform to be very great, the fits being much lessened in frequency and violence, the comatose state rendered shorter and less profound, and the period of great mental aberration after the cessation of the coma considerably diminished.

2nd. In the maniacal or delirious state due to hysteria, epilepsy, or uræmia, and also in cases of aggravated chorea preventing sleep, chloroform is of immense service, particularly i morphia or some other somniferous remedy is given to the patient, either by the hypodermic method or by injection into the rectum, as soon as chloroform has produced sleep. Dr. Briquet states that nine times out of ten he has stopped attacks of hysteria by inhalations of chloroform. I have not been quite so successful as Dr. Briquet in cases of hysterical convulsions, but attacks of delirium and mania due to hysteria are almost always quickly brought to an end by chloroform.

3rd. In the case of a gentleman who had pretty regular weekly attacks of epilepsy, I once employed chloroform by inhalation, almost without interruption for two or three days successively, with the object of preventing the expected fit or fits. It was of the greatest importance in that case to prevent a fit, as the patient in a preceding attack had fractured and dislocated one of his arms. The inhalations of chloroform saved him from the expected attack, and the callus had time to be formed before he had another fit. This patient was seen with me by Mr. Robert Dunn.—*Lancet, March* 10, 1866, *p.* 247.

26.—ON THE USE OF ICE BAGS IN CONVULSIONS AND IN CHOLERA.

By Dr. John Chapman.

[Dr. Chapman's theory is that cold and heat applied to the spine produce definite and constant effects. He considers that ice along the spine increases the general circulation, stops the cramp of voluntary and involuntary muscles, proves an effective remedy in epilepsy and other convulsive affections, cures sea sickness, restrains the sickness of pregnancy, arrests diarrhœa, recovers patients from the cold stage of cholera, and, finally, promotes menstruation. On the other hand, heat along the spine lessens the general circulation, overcomes congestion in all parts of the body, lessens fever, restrains hemorrhage, and lessens or arrests the menstrual flow.]

First as to the power of ice along the spine to influence the spinal cord :—The late Dr. Todd, who entirely misconceived the nature of the influence on the general circulation which is exerted by ice applied along the spine, and who stated it to be exactly the reverse of what it is, testified that "cold has a powerful effect in depolarising the spinal cord. I have tried it," he says "with great advantage in tetanus, in laryngismus, and in the convulsions of children. . . In the cases in which I have tried it," he adds, "I have found it manifestly beneficial in diminishing the intensity of the spasms." [Unfortunately, Dr. Todd's false doctrine that ice along the spine "has a very powerful influence in diminishing both the force and frequency of the heart's action," precluded him from freely availing himself of this remedy.]

In the Medical Times and Gazette for March 12, 1864, there is a report of a case of "Emprosthotonos(?)," treated by Dr. James Edmunds by means of ice. The following is a summary of that report :—Jan. 21.—A married woman, 28 years old, healthy, intelligent, and free from flightiness or hysteria, went to bed quite well ; on rising next morning at 7.30 her fingers were slightly benumbed ; between 9 and 10 they suddenly turned in ; the flexion became rapidly more intense ; a cramp-like pain in the arms ensued ; and soon the lower limbs from the knees downwards became affected in a similar way, but less severely. The symptoms continued to increase throughout the day, and at 10 p.m., when Dr. Edmunds first saw the patient, there was extreme and rigid flexion of her hands and arms ; her head was so drawn forwards that the backs of the wrists were fixed close to her mouth ; her spine was bent forwards, and her knees were bent and drawn upwards. Her forearms were hard and rigid, and the wrists were so fully flexed that her hands were forced open, and her legs were similarly affected, but in a much less degree. Her neck became turgid, her respiration somewhat constrained, there was an expression of great anxiety and pain upon the face, and the lips were sufficiently retracted to show the teeth. Her mind and manner continued clear and rational ; but the tearing cramp like pain forced from her a continual moaning scream. During the evening she suffered so much that her screams could be heard across the road. She was becoming rapidly exhausted, and "it was manifest that unless speedily relieved she would die either from exhaustion or from spasms of the respiratory muscles." Between 11 and 12 a.m., she was first seen by Dr. S. C. Griffith, who ordered her aperient medicine. After taking one dose she was slightly sick. At 1 p.m. Dr. Webber saw her in consultation, "noted her pulse as slow and weak, about 60," ordered sulphuric ether and anti-spasmodics at once, and a dose of morphia in a few hours if the pain should not abate. The morphia was given, and afterwards "all her symptoms were aggravated." Dr. Edmunds ordered ice to the spine. Her husband applied it continuously to the whole length of the spine for seven hours. The screaming and moans were at once mitigated by the ice, and within about half-an-hour from its first application had ceased entirely. In two hours her feet became flexible. About 8 a.m. on the 23rd the spasms had so far subsided that she could open her hands, but the numbness remained. Her pulse had increased in volume, and had risen to 76. On the morning of the 24th the numbness entirely disappeared; she had no return of cramps or spasms, but a little subacute bronchitis and hoarseness hung about her for ten or twelve days. She then regained her usual health.

At the close of his report, Dr. Edmunds observes: "I had seen Dr. Chapman's *brochure* on the subject of his discovery, and also his paper in the Medical Times and Gazette, but thought the idea too pretty to be anything more than a plausible theory, until my own child being in great danger from an obstinate laryngismus connected with dentition, I tried the ice-bag to the cervico-dorsal portion of the spine, at the suggestion of Dr. Ramskill, and it has certainly done more to keep off the strangling attacks than anything else.

"Dr. Ramskill has not only adopted this treatment largely, but speaks of it to me with the greatest confidence in the treatment even of epilepsy, and in his practice it seems to be the means of doing great good, and in many cases, of keeping the fits almost entirely in abeyance."

In the Medical Times and Gazette for May 28, 1864, there is an account of a case of paralysis relieved by ice, by F. Broughton, F.R.C.S., Surgeon-Major, Bombay Army. The following is a summary of his report:—In October, 1863, he visited a poor girl, J. D., aged 21, who had been confined to her bed for upwards of a year. There was entire loss of sensation and motion from a little above the knees downwards, in both inferior extremities. She could not turn herself in any way, but was treated by her mother and sister in every respect as an infant. There was neither wasting nor rigidity of the muscles of the legs: but the feet were both stiff and attenuated.

Pricking and cutting the skin of both legs produced neither pain nor twitching. No curvature of the spine could be detected; but there was extreme tenderness on pressure in the lower cervical and in the dorsal region. Her general health was good; menstruation natural; had never shown symptoms of hysteria; but had been long troubled by weakness and pain in the back, which had caused her to limp on one leg. The pain in the back increased, and, as up to the period when Mr. Broughton first saw her much and varied treatment had been resorted to in vain, she and her parents had given up all hope of recovery. Having had his attention directed to the agency of ice in such cases, by reading my paper on the subject, he ordered it to be applied to the spine daily during two hours, each application being followed by prolonged friction down the spine to the extremities. At the end of ten days a little sensation was experienced in her feet. This was soon followed by slight motion, and by the middle of November she could turn herself in bed. The treatment was continued, and by December 1st she was able to stand and move about the room. She improved so rapidly that "on Christmas-day," he says, "I had the satisfaction of meeting her walking in the streets of the village in which she resides." He adds: "I saw her on February 29th, as I was then leaving the neighbourhood, and found her quite able to conduct the ordinary duties of a cottage household." No medicines were employed in the treatment of this case.—*Medical Times and Gazette, Dec.* 2, 1865, *p.* 612.

27.—CHOREA TREATED BY CALABAR BEAN.

[Dr. Ogle, of St. George's Hospital, has been trying the Calabar bean in the treatment of chorea. The first case was a girl 20 years of age. She did not appear to gain ground under any plan of treatment previously tried.]

He directed her to begin with half a drachm of the tincture (supplied by Bell to the hospital, and having the strength of one drachm of the bean to one ounce of rectified spirit of wine) administered three times a day in water, and increased it by ten drops at a dose, *making no alteration in diet.* She very quickly improved in muscular steadiness, and on September 10 the nurse reported "she has cut her own food to-day for the first time." The dose of the bean was raised to ʒj. three times a-day; and on the 19th the wine was omitted. She so quickly and so satisfactorily recovered that she was discharged from the hospital as almost quite well. On inquiry of the nurse of the ward (Roseberry), Dr. Ogle learned that the patient made her appearance about the middle of November, and that she had become so stout and looked so well that she was hardly recognised by the nurse. She had quite lost all choreic symptoms.

Case 2.—Hannah W., aged 13, pale in face, but otherwise healthy, became an out-patient September 15 last. She was affected by chorea, which was mainly but not entirely confined to the "right" side (both arm and leg). The speech was much affected ; the tongue feeling, as she described it, "too large for the mouth." The pupils were equal, and natural in character. Symptoms of the disease had shown themselves for six weeks. She had never had rheumatic or scarlet fever or other severe disease, and had not suffered from worms, &c. No other member of the family had been similarly affected.

Dr. Ogle ordered her twenty minims of the tincture of Calabar bean in water three times a-day, making no alteration in diet, and giving no other remedy.

On the 22nd, when she came, the mother said the "shaking was on the other side" now. The pupils were rather enlarged. The dose was increased to thirty drops three times a-day.

On the 29th the statement was that she was "stronger," but she still "rolled the tongue," and the speech was still affected. The tincture was then increased to forty drops three times a-day.

On October 6 it was stated that she eat better, but that there was still a difficulty in opening a book with the hands. The pupils were natural ; sight good.

She went on with the same dose of the Calabar bean until November 3, when it was stated that there was no shaking whatever. Food was taken with a natural appetite. The pupils appeared to be natural. She was discharged as being quite well.

Dr. Ogle stated that he had one or two other patients with chorea under his care who were at present taking the Calabar bean, and that alone, apparently with marked benefit. At the same time, he had had some cases under his care in which no good appeared to attend its use. He considered that choreic patients who were out-patients were better adapted for a trial of the services of the bean than those who were in-patients, as in the latter the use of the remedy becomes complicated by the changes of food, rest, and care incident to residence within the hospital. Dr. Ogle proposes to try the eserine, or active principle of the Calabar bean, the alkaloid thereof, in chorea as well as in other forms of disease.—*Med. Times and Gazette, Jan.* 13, 1866, *p.* 36.

28.—SUBCUTANEOUS INJECTION OF NARCOTICS FOR NEURALGIA.

By Dr. John W. Ogle.

Subcutaneous Injection of Aconitine and Morphia for Neuralgia.—Mary K., aged 22, became an out-patient September 11, 1865, suffering from neuralgic pain, which she had had for some weeks, down the back of the left thigh and in the muscles forming the left buttock. This was at times excruciating, and could not be connected with disease of any organ. She was, however, not a very robust person.

Dr. Ogle at once ordered the subcutaneous injection of $\frac{1}{40}$th of a grain of aconitine, and enjoined that she should come again to the hospital in three days' time, and have $\frac{1}{30}$th of a grain of aconitine injected. Valerian, steel, and quinine were also prescribed.

She experienced a certain amount of relief from the subcutaneous injection, but not so much as was expected; so that on the 18th half a grain of acetate of morphia was injected. After this she complained of feeling "very queer" for three hours, and subsequently passed a restless night, "sleeping and waking," as she said. The next day she felt "weak;" but for two or three days decidedly less pain was experienced. On the 25th the pain had returned very acutely, and therefore $\frac{1}{25}$th of a grain of aconitine was injected. At the end of a week she returned, saying she had received very great good from the last injection, and had been quite free from pain ever since. She was accordingly discharged, as being no longer in want of treatment.

Subcutaneous Injection of Atropia and Morphia in Neuralgia of the Thigh.—Emma G., a rather delicate and pale young woman, was admitted under Dr. Ogle's care on September 6, 1865, with acute pain down the back of the left thigh of a persistent character. There seemed to be no actual disease of any organ. It appeared that she had been previously in the hospital with a similar attack, which was benefited by subcutaneous injection of some sedative. Accordingly Dr. Ogle at once ordered $\frac{1}{30}$th of a grain of atropia to be injected beneath the skin of the thigh and a senna draught to be given. This produced no good result whatever, and then a draught with half a grain of morphia every night was prescribed, and in a day or two quinine in 7-grain doses twice a day and occasional pills with colocynth and hyoscyamus.

On the 18th Dr. Ogle ordered half a grain of morphia to be injected subcutaneously. This gave a considerable degree of relief, lasting a whole day, and on the 25th it was repeated. She quickly improved after the latter injection, and on Oct 2 was discharged free from pain. It is right to state that she also had gone on with her quinine daily, and had been taking ordinary meat diet.

Neuralgia of the Face (Infra-Orbital Branch of the Fifth Cranial Nerve)—Relief from Steel, Quinine, and Subcutaneous Injection of Morphia, &c.—Anne V., aged 58, a widow, thin, and delicate looking, had for four months been subject to acute pain extending from below the right orbit to the middle of the upper lip, which was rather swollen. She became an out-patient January 9, 1865. This was wont to come on suddenly, lasting a minute and a-half, as well during the night as day. The pain was described as being like scalding with hot water or violent pricking, and was brought on by lifting up the lip with the finger, sucking, swallowing spittle, eating and drinking, &c. Very rarely the pain was also felt *above* the orbit. Moreover, all along the course of the pain the surface of the skin was *exquisitely hyperæsthetic.* There was a slight degree of drooping of the right upper eyelid, but there was no paralysis of the facial muscles, or of those of the arms or legs, &c. The pupils were natural. There were no teeth in the upper jaw, as they had been extracted or had fallen out from being decayed when young.

The patient thought that her pains began with "cold and great discharge from the nose." The mental powers seemed intact. For several weeks she was treated with steel and five-grain doses of quinine. Belladonna ointment was applied to the painful part, but the patient thought that this made her worse at the time; perhaps it was from the friction. On the whole she much improved, the pain still being described as "jarring," "pricking," "throbbing," but in the intervals of pain she was much stronger and more comfortable. As this appeared to be especially so after the use of night pills containing half a grain of acetate of morphia, the same quantity of morphia was injected (February 27) beneath the skin of the right cheek. This gave great temporary relief. She afterwards took quinine with four minims of the tincture of aconite. On March 20 one grain of morphia was injected subcutaneously. Since that time the patient has not presented herself at the hospital.—*Med. Times and Gazette, Feb.* 17, 1866, *p.* 174.

DISEASES OF THE ORGANS OF RESPIRATION.

29.—ON THE NATURE, CAUSE, AND TREATMENT OF TUBERCULOSIS.

By Dr. HORACE DOBELL, Physician to the Royal Infirmary for Diseases of the Chest, &c.

[The hypothesis set forth at the commencement of the following paper is the result of much study, and is one interesting alike from its novelty and its importance.]

Tuberculosis is due to defect in the action of the pancreas on the fat taken as food (especially the solid fat). The supply of properly prepared fat is cut off from the blood: 1, by the fats not being brought into a proper condition by the pancreas; 2, by loss of absorbing power in the small intestine, due to the contact of unhealthy pancreatic juice and of defectively prepared food with its mucous membrane. Thus the blood becomes deficiently and defectively supplied with fat-elements from the food; is unable to afford those required for direct combustion; does not replace those taken up during interstitial nutrition; but, on the contrary, takes up more to compensate the deficient supply from the food. This having gone on up to a certain point, the fat-elements of the albuminoid tissues are seized upon, and these tissues are minutely disintegrated in the process. This disintegrated albuminoid tissue is nascent tubercle; and this process of disintegration is tuberculisation.

Tuberculisation will take place first wherever the following combination of conditions is most marked.

1. Greatest activity of interstitial nutrition.

2. Smallest amount of fat-elements able to be spared by the tissues.

3. A double process going on, consisting of: (*a*) ordinary interstitial nutrition in albuminoid tissue; (*b*) interchange of oxygen and carbonic acid, or carbonaceous matter through this tissue.

The tubercle thus formed may be allowed to remain on the spot where it is formed, constituting a *primary deposit at the point of origin*, and this will especially occur when formed under the three conditions just specified; or, it may not be allowed to remain on the spot where formed, but be at once carried away by the lymphatics in a minutely divided condition, and either arrested in the lymphatic glands or carried on into the blood to be deposited from it, constituting a *primary deposit distant from the point of origin;* or, having been primarily deposited in either of these ways, it may be taken up by the lymphatics and deposited in the lmyphatic glands, or carried into the blood and deposited from it, constituting a *secondary deposit.*

In the *advance* of these diseased processes, any part of the body in which nutrition is going on in albuminoid tissue may become both the source and seat of tubercle; and any part, whether albuminoid or not, if supplied with lymphatics or blood-vessels, may become the seat of tubercle.

Pure tuberculosis commences when fats, properly acted upon by the pancreas, cease to pass in normal proportions into the blood.

Tuberculisation commences when albuminoid tissue is abnormally seized upon for its fat-elements.

A secondary state is supperadded when tubercle has been carried into the circulation, which constitutes tuberculæmia or tuberculous blood-poisoning.

Tuberculosis may stop short of the production of tubercle (tuberculisation), if the normal supply of fat, properly acted upon by the pancreas, be restored to the blood before the albuminoid tissues have begun to be disintegrated; and it will not return so long as the supply is kept up to a normal standard. Tuberculosis may stop short, after the production of tubercle, if the normal supply of properly prepared fat be restored; but, in this case, the tissues must have become so poor in fat-elements that it will be difficult to escape the formation of fresh tubercle in the defective tissues, while the reconstruction of normal tissue is going on. A very rapid and superabundant supply of properly prepared fat to the blood will, therefore, be required.

The balance of fat-elements in the blood and tissues having been restored, tuberculosis will cease; but tuberculæmia with all the difficulties due to the presence of foreign decomposable and absorbable matter in the tissues and blood, may remain.

In progressing tuberculosis, after the occurrence of tuberculisation, tuberculæmia is added to and combined with the original disease. An ordinary case of consumption is a combination of these three states.

If the second portion (*b*) of the third condition especially favouring tuberculisation—viz., "an interchange of oxygen and carbonaceous matters through tissues nourished by blood deficient in fat-elements"—can be provided for by supplying carbonaceous matters to the circulating medium of the part otherwise than by the normal processes and channels, a temporary protection from tuberculisation will thereby be given to the tissues through which the interchange occurs.

If this part were previously the one in greatest danger of tuberculisation, it will for a time give precedence to the part not so protected; *e.g.*, the lungs may thus be protected by the introduction of oleine through the portal system, the small intestine and mesenteric glands taking precedence in the order of tuberculisation from the want of such protection.

But as nothing can permanently protect the albuminoid tissues from disintegration except solid fat properly acted upon by the pancreas, the protection by the means above described is only temporary; and, unless the normal conditions be soon restored, the previously protected part will become tuberculous at last. By these means, however, the succession in which parts ordinarily suffer may be changed; and if the normal state be restored in time, the part temporarily protected may permanently escape disease.

The defective function of the pancreas causing defective action upon fats may be temporary or permanent, and a tendency to such defect may be hereditary.

When the normal function of the pancreas had been restored, the mucous membrane of the small intestine may not have recovered its absorbing powers, and thus the fats, though properly prepared by the pancreas, may not enter the blood.

Defect in the function of the pancreas may be produced :

1. By any cause which for a prolonged period greatly reduces its activity, by diminishing the normal demand for carbonaceous matters in the blood.

2. By the action on the nervous system of powerful or prolonged depressing influences.

3. By inflammatory and other abnormal conditions of neighbouring parts.

4. By prolonged loss of absorbing power in the small intestine by which the function of the pancreas is rendered useless.

The normal function of the pancreas may be restored (within certain limits as to the duration of the existence of the cause of its defect) by means which make a healthy demand upon its active functions, remove the depressing influences from the nervous system, and permit the absorption of properly prepared fats by the mucous membrane of the small intestine.

This hypothesis having been constructed step by step to explain the phenomena of tuberculosis, I feel safe in asserting that there is hardly a well established fact connected with the origin, course, terminations, and therapeutics, of tuberculosis, which cannot be more satisfactorily explained by this hypothesis than by any other yet suggested. I have carefully compared all its parts with the best established views in modern physiology; and, so far as my knowledge and judgment are competent to decide, there is nothing essentially inconsistent with these views in any portion of it. At the same time, I am quite aware that there are many important points in physiology, unavoidably involved in every theory of nutrition, whether normal or abnormal, on which our knowledge is, at the best, vague and unsatisfactory. All that can be expected, therefore, in this respect, is that conditions shall not be required by a theory of morbid nutrition which are forbidden by the knowledge we have of normal nutrition. I believe it will be found that I have studiously kept within these limits; but, as I have already said, I am conscious of my deficiency in profound physiological and chemical attainments; and I am, therefore, the more desirous that my views shall be examined by those who possess these qualifications, in order that their truth may be strictly tested.

It is not my intention, then, to enter into a discussion of the purely physiological and chemical details of this hypothesis, but rather to direct attention to some of the most striking points in which it is consistent with the clinical history and pathology of tuberculosis, and especially to those in which it reconciles and explains apparently incongruous phenomena.

In the first place, it is necessary to impress the importance of the position occupied by fat in the processes of life; and I cannot do this better than by quoting the following statements by recognised physiologists, in which my own opinions are embodied.

"In considering fat as an important agent in the various phases of the metamorphosis of animal matter, we cannot, however, refer its action solely to mere contact or catalytic force; but we are constrained to assume that it co-operates in the metamorphic action, and experiences metamorphoses, combinations, and decompositions." (Lehmann's *Chemistry*, vol. I, p. 269.)

Under certain circumstances and for a certain time, "animals can be maintained by a nourishment from which fat is entirely excluded"; and, when the increase of fat in their bodies is in excess of the substances soluble in ether taken as food, "the formation of fat is due to a process of life." (Letter from Professor Liebig to the author, April 3, 1853.)

"In all the forms in which albumen naturally occurs, it is combined with fatty matter." (Kirkes's *Physiology*, 3rd edition, p. 12.)

"The usual function of fat is unquestionably, like that of starch or of sugar, to keep up the heat of the animal..........We require the combustion of all the non-nitrogenous constituents of food to enable us to account for animal heat.........But it is, nevertheless, a fact that fat is always present in healthy muscles; and it is desirable to consider its relation to muscular action...........The experiments made by Bidder and Schmidt on starving cats, and by Bischof and Voit on a starving dog, throw light upon this subject....When Bischof and Voit supplied their starving dog with fat, the waste of the body, as evidenced by the lessened amount of urea excreted, was diminished, because the fat supported the respiration, which before had partially to depend on wasting tissues. The fat cast over them a protective influence, and limited their waste to the support of their own dynamic functions; and in this fact would seem to be the use of fat after it is stored up in the muscle. We allude to its chemical use; for its mechanical advantage in lessening friction and its possible histogenetic employment in the formation of cells are not under consideration.........A man in ordinary health and activity wastes daily 1750 grains of dry flesh or 7000 grains of fresh muscle, which would contain 150 grains of fat .... We refer to fat distributed in and inherent to healthy muscles, and not to masses of adipose tissue...... The chemical use of fat deposited within muscle may be to protect it from the assaults of oxygen during repose. A muscle, when at rest, gives out carbonic acid, which is, no doubt, partly due to the oxidation of its effete particles, but also to the oxidation of fat .... Tissues may, and do, evolve heat by transformation when required to do so." (Dr. Lyon Playfair, *The Food of Man in relation to his Useful Work:* 1865.)

A part of the heat necessary to maintain a constantly elevated temperature in birds and animals, "depends upon the *direct* combination of certain elements of the food with the oxygen of the air by the combustive process. The quantity of carbonic acid that is generated directly from the elements of the food, seems to vary considerably in different animals and in different states of the same individual......In man and other animals which can sustain considerable variations of climate, and can adapt themselves to a great diversity of habits, the quantity of carbonic acid formed by the *direct* combination of the elements of the food with the oxygen of the air differs extremely under different circumstances. It will serve as the *complement* of that which is formed in other ways." (Carpenter's *Principles of Physiology*, 6th edition, p. 265.)

"The fatty matters of the blood are obviously destined to furnish the contents of the adipose and nervous vesicles, whilst their presence seems also to be required in the early stages of the production of cells generally. One of the principal sources of their expenditure, however, is that combustive process by which the heat of the body is maintained; and the amount deposited in the tissues as fat may be looked upon as the surplus of the quantity ingested that is not thus consumed......... The question of the capability of animals to produce fat has been much disputed. There seems, however, no reason for doubting that it may be generated, not only from the hydrocarbonaceous constituents of the food (sugar and starch), but also from the splitting up of the albuminous compounds." Carpenter, *op. cit.* p. 341.)

"It is clear that fat exerts a protective influence over the albuminous tissues, sparing their consumption or oxidation by its own greater affinity for oxygen.........When fat is added to the food, its direct combustion takes up the oxygen, and prevents its action on the nitrogenous tissues." (Carpenter, *op. cit.*, p. 332.)

In the second place, I must point out that, although reliable observations on the blood in phthisis are very scarce, those which we have are consistent with my hypothesis. "In this disease, the saponified fatty matters of the blood decrease perhaps more than in any other." (Becquerel and Rodier's *Animal Chemistry*, Speers's translation, 1857.)

In the next place, all reliable analyses and observations of the nature of tubercle itself give results consistent with my statement, that it is albuminoid tissue disintegrated by the combustion of some of its fat elements.

From the analyses of Scherer, "tubercle may be regarded as protein from which five atoms of carbon, one of hydrogen, and one of oxygen, have been removed." (Simon's *Chemistry*, translanted by Dr. Day, p. 479.)

"Chemistry, equally with the microscope, has failed to point out any essential difference between 'grey miliary' and 'crude yellow' tubercle. Chemists are agreed in regarding tubercle as consisting of some modified protein compound or compounds with fat and earthy salts." (Dr. Fuller, *Diseases of the Chest*, etc., p. 355.)

"It exhibits no tendency to form perfect cells, but rather abortive corpuscles, which form slowly, and slowly break down. Elementary molecules are not formed capable of further development. . . . The tissue which is occasionally seen mixed with it in expectorated matter, is disintegrated lung (Andrew Clark), and only means that so much pulmonary structure has been destroyed." (Dr. Pollock, *Prognosis in Consumption*, p. 111.)

"The destructive change which takes place in tubercle is not a vital act, but is determined by a conversion of the chemical components arising out of an interchange of the elements." (Rokitansky, Sydenham Society's edition, vol. i, p. 299.)

In the fourth place, it must be observed that not only all the symptoms of confirmed phthisis, but the earliest appreciable symptoms of impending tuberculisation, upon which physicians are generally agreed, are entirely consistent with my hypothesis. These latter have been briefly and pointedly stated by Dr. Pollock in his recent work. "A pre-existing disorder of health is invariably present before there is any evidence of deposit in the lungs.........These are symptoms not referable to any disease of the lung, so far as the function of respiration is concerned, but clearly indicative of a general disorder of the system......... The earliest of all this group is emaciation." (*Op. cit.*, p. 39.)

The next point to which I would direct attention is the occurrence of hemoptysis in the early stage of phthisis. It so often happens that the first occurrence of hemoptysis is followed by a steady descent of the patient, that, *popularly*, the hemoptysis is looked upon as the cause of consumption. Although this is proved to be erroneous, it is a fact that, in a large proportion of cases, the first attack of hemoptysis, even though slight in amount, is followed by marked symptoms of phthisis, and especially by loss of weight. The first medical report of the Brompton Hospital (1849) shows that hemoptysis occurs in 63 per cent. of all cases of phthisis, and is more frequent in the first stage, as 3 to 1. Dr. Pollock says: "I have observed 351 cases of profuse hemoptysis, of which 204, or more than one-half, occurred in the first three months of the illness." "An early profuse hemoptysis is to be regarded as a bad prognostic; the softening being likely to take place early, and the remission in the progress of the disease to occur only at a period when already much structural mischief has taken place. "The prognostics to be derived from slight repeated hemoptysis are, on the other hand, not favourable. this being the commencement of the ordinary form of phthisis." (*Op. cit.*, p. 139.)

This is entirely in accord with my hypothesis. For, so long as there is a sufficient supply of fat in the system to protect the albuminoid tissues, there is no reason why tissue should be invaded; but, as soon as this ceases to be the case, the first breaking up of organic structure will occur; and this is the moment at which hemoptysis may be reasonably expected to take place for the first time. The hemoptysis, therefore, although in no sense the cause of the tubercle or of the emaciation, may, in truth, often mark the point in the case at which the formation of tubercle begins, and also the point from which emaciation and other general symptoms of disease must advance in a marked degree, until the progress of tuberculosis is stopped.

A sixth and still more interesting point is the relation of tuberculosis to the menstrual function, with respect to the effect upon the carbon in the system. Menstruation in health has a definite relation to the quantity of carbonic acid discharged from the system by other means. From the occurrence of puberty to the cessation of menstruation, so long as healthy nutrition is maintained, everything which arrests menstruation increases the discharge of carbonic acid from the lungs; and the recurrence of menstruation reduces that discharge. It is evident, therefore, that the menstrual flux is a means of eliminating carbon. Now, it is well known that, previous to the appearance of local symptoms of tubercle, the catamenia usually become irregular, and gradually diminish in quantity until, as the disease goes on, they entirely cease; that permanent arrest of menstruation is a usual occurrence in confirmed and progressing tuberculosis; that any prolonged arrest in the disease is usually attended with a restoration of menstruation; that, when menstruation coincides with confirmed and progressing tuberculosis, all the symptoms of the disease are usually increased at the monthly period; that the cessation of menstruation during pregnancy is commonly accompanied by quiescence in the symptoms of tuberculosis; that a large number of cases of phthisis commence at the age of puberty; and (which appears at first very curious) that at this age, among those attacked, there is a large preponderance of males over females—"123 to 90." (Pollock, p. 295.)

As I read these facts, they have the following meaning. In females, the balance of their nutritive functions is so arranged that, at the age of puberty and during the whole generative period, there shall be, normally, a surplus of carbon in the system. At the age of puberty, this superfluous carbon protects them from some of the dangers of tuberculosis to which boys, who have no such surplus to fall back upon, are exposed. Thus, a girl at the age of puberty, deprived of some of the normal supply of properly prepared fats by a temporary arrest of the function of the pancreas, simply suffer a delay in the appearance of menstruation, appropriating her surplus carbon for the protection of the tissues; whereas a boy, similarly placed, has no such means of escape.

The disappearance of the catamenia when tuberculosis has set in is explained by the deficient supply of fat to the blood cutting off the surplusage of carbon for excretion, The latency of tuberculosis, when the catamenia are retained by pregnancy, is explained by the saving thus temporarily effected in the expenditure of carbon. The restoration of the catamenia during periods of arrested tuberculosis is the expression of a return to the normal proportion of carbon in the system. With this clue, many other analogous phenomena may be easily explained. It is important to remember that, although the number of reliable experiments on the quantities of carbonic acid exhaled by the lungs in tuberculosis, as compared with the quantity in health, is not large; and that such experiments are open to many sources of fallacy; yet, so far as they go, they are to the effect that the exhalation of carbonic acid from the lungs is diminished in tuberculosis, which is, like the above facts, in accordance with my hypothesis.

[The order in which primarily tuberculisation usually takes place in the principal organs, is as follows:]

In adults and children, the lungs are by far the most frequent seat of tubercle. In adults, the small intestines and mesenteric glands are the most frequent seat of tubercle except the lungs. In children, the bronchial glands are affected with tubercle more frequently than any other part except the lungs, and they may be tuberculous without the lungs being so. After the bronchial glands, the small intestines and mesenteric glands are the most frequent seat, as in adults.

The lungs are the organs, in adult life, in which, *par excellence*, the required combination of conditions for precedence in tuberculisation exists. The small intestine and mesenteric system are the parts most nearly resembling the lungs in this respect.

It is hardly necessary to point out that there is no other part of the body traversed by fluids essentially the same as those which traverse the lacteals, mesenteric glands, thoracic duct, and pulmonary artery; and that there are no other parts of the body at which the relations between fats acted upon by the pancreas, albuminoid tissue, and oxygen, are so similar as in the small intestine and mesenteric system, and in the lungs.

In the lungs, the oxygen is in the air and in the nutrient capillaries; in the intestine and mesenteric system, it is in the blood of the capillaries.

[Dr. Horace Dobell's hypothesis also affords a satisfactory explanation of the variable course of tuberculosis in different cases.]

1. A sudden, almost complete, or total suspension of normal pancreatic secretion, accounts for acute tuberculosis.
2. A loss complete suspension or perversion of the function of the pancreas accounts for chronic tuberculosis.
3. Either of the above changes of pancreatic secretion occurring intermittently, accounts for recurrent tuberculosis.

I have next to speak of that portion of the hypothesis which assumes a change in the absorbing power of the small intestine, by which the passage of fats into the blood may be prevented after the function of the pancreas has been restored.

That the continual passage, through the delicate absorbent apparatus of the intestinal mucous membrane, of a depraved pancreatic secretion, and of food that has not undergone those changes which fit it to be brought into contact with the absorbing surface, must damage the apparatus, is a conclusion which analogy justifies us in assuming to be correct. That such damage does occur in the alimentary canal of the tuberculous, is amply proved by a pathological research; and the extreme difficulty of identifying, after death, delicate changes in the condition of mucous membranes, renders it certain that such changes may continually occur without being recognised in a *post mortem* examination.

Bernard found that, in dogs who had been deprived of their pancreas, the fæces commonly became streaked with blood, as from ulceration of the intestines. (*Mémoire sur le Pancreas,* 1850.)

Bidder and Schmidt observed, in their experiments on the lower animals, that inflammation of the mucous membrane of the intestine at once prevented the absorption of fatty chyle by the lacteals.

To say the least, it is in the highest degree probable that perverted pancreatic secretion, and food improperly prepared for absorption, passing over the mucous membrane of the intestine from day to day, do, sooner or later, so damage its absorbing function as to add an additional obstruction to nutrition by the lacteal system; and, as Bernard has shown that the general perturbations of the economy, and the slightest inflammation in the neighbourhood of the pancreas, especially in the intestine, pervert the properties of the pancreatic juice, any damage done to the mucous membrane of the neighbouring intestine by imperfect pancreatic action must react upon its cause, and throw a double difficulty in the way of restoring either the pancreas or the mucous membrane to a healthy condition.

Treatment.—Assuming my hypothesis to be correct in its chief parts, the following are the principles of treatment which it indicates.

1. If the case be so far advanced that it is considered hopeless to aim at restoring the function of the pancreas and absorbing power of the intestine, two principles must guide our treatment.

A. To supply the greatest amount of fat to the blood that is possible by other means than through the action of the pancreas.

a. By giving pancreatic emulsions of solid fat, or, if these be rejected, pancreatic emulsions of oil, in the hope that some absorbing powers for such matters may yet remain in the digestive tract.

b. Chiefly by giving olein, with a view to its absorption by the portal system of vessels; and by rubbing it into the surface of the body and limbs, with a view to its absorption by the skin.

c. By giving highly albuminous food in conditions in which it may be most easily utilised, so as to supply an excess of material as a source of carbon by disintegration.

B. To save the albuminoid tissues from disintegration to the greatest possible extent.

a. By supplying a surplus of carbohydrates, so as to economise as much as possible the consumption of hydrocarbons.

b. By diminishing the proportion of oxygen in the air presented to the lungs.

c. By diminishing the demand for the generation of animal heat, by supplying it artificially.

d. By reducing respiratory and cutaneous action to the lowest point consistent with maintaining what remains of appetite and digestive power.

It is obvious that the persistence in these means must be considered utterly at variance with curative intentions, and simply as the expedients best fitted to prolong the process of gradual death.

2. If the symptoms be only what is commonly called "premonitory"—that is, if they be those of commencing tuberculosis, and no reason or sign be discoverable which justifies the suspicion that tuberculisation has commenced; if a sufficiency of fat-elements remain, without calling upon the albuminoid tissues—the principles of treatment are quite opposite to those last detailed. They are now entirely curative in their intention.

A. To restore the function of the pancreas as quickly as possible, by placing the patient under those conditions which call for pancreatic action.

a. An atmosphere rich in oxygen.

b. A climate at once cold and dry.

c. Exercise of the respiratory and cutaneous functions.

d. Cheerfulness of the mind and quiet to the emotions; exhilaration of the animal spirits.

e. A good mixed diet, easily digested, and free from substances irritating to the mucous tract.

f. Pancreatic emulsions of solid fat and pancreatic juice in quantities sufficient to ensure protection to the albuminoid tissues while the process of restoration is going on in the pancreas, but not in such quantity as to entirely supplant the natural pancreatic function and thus to counteract the measures adopted for calling it into action.

These two classes of cases represent the two extremes in the history of tuberculosis—the greatest and the least hope of cure.

3. A third class of cases, intermediate between these extreme points, represent the bulk of all those brought under the care of the physician. They require a combination of the two principles of treatment already stated. The nice adjustment of these two plans of treatment—the hopeful and the hopeless, the palliative and the curative—calls for all the intelligence and judgment that a medical man can possess.

If tuberculisation, even in the smallest degree, be going on, the first and most urgent need is to supply fats. Olein should be given for absorption by the portal system, and thus to afford, so far as it is able, materials for combustion. But, above all, fats acted upon by pancreatic juice are called for, and must be supplied until the deficiency is removed, the just balance restored, the process of tuberculisation stopped. Until this point is arrived at—until the balance is turned in favour of the albuminoid tissues—everything which favours the reception of oxygen into the blood, everything which increases the wear and tear of the body, everything which calls for the generation of animal heat, directly favours tuberculisation, and precipitates the patient into the very catastrophe we wish to avert.

On the other hand, so soon as the balance has been turned in favour of the albuminoid tissues, and tuberculisation has been artificially arrested, everything which postpones the restoration of the function of the pancreas directly favours the conversion of a temporary into a permanent disease. But moderation is essential to success. Too great and too sudden a demand upon a weak and defective pancreas may only paralyse the organism in its attempts at restoration of function. It is, however, impossible to magnify the importance of periods of arrested tuberculisation, whether artificially or naturally produced. They are the bright opportunities for the permanent restoration of pancreatic function. Too often these opportunities are wasted. The patient and his friends, pleased with the marked improvement that has taken place under some plan of treatment, either continue this treatment long after its proper time and place have passed by, or give up treatment altogether, content to wait till signs of active disease return. In either case, an opportunity for establishing a permanent recovery is thrown away, and perhaps it may never return.

It must not be forgotten that, when tuberculisation and tuberculosis have been stopped, when the function of the pancreas has been restored and the intestinal mucous membrane brought back to a normal condition, there may yet remain tuberculæmia. From time to time, fresh blood-poisoning may occur from the absorption of deposited tubercle ; and, if there be any considerable quantity deposited, this chronic blood-poisoning may become a tedious and dangerous disease. The absorbed tubercle may be again deposited and again absorbed, and thus keep up a ceaseless repetition of morbid changes somewhat analogous to those witnessed in pyæmia. Treatment directed to the blood-poisoning will, therefore, be imperatively called for in most cases of tuberculosis which have advanced to the stage of tuberculisation.

I have long been aiming at testing my views regarding the action of the pancreas in tuberculosis by a crucial experiment ; but the difficulties in the way of artificially supplying pancreatic juice to a number of patients were so numerous that they were not finally overcome till the summer of 1863. With the valuable assistance of my friend Mr. Heathorn, who had long been working with me at the subject, I succeeded in obtaining a pancreatic emulsion of fat sufficiently palatable and in sufficient quantity to permit of my beginning my crucial experiments by administering it to patients suffering from tuberculosis at the Royal Infirmary for Diseases of the Chest. For some time our supply was too fluctuating to allow of the experiments being carried out in a manner that I thought justified me in making the results public ; but in the summer of 1864 I was able to draw up the first of the three reports which have since appeared in the *Lancet.* This was published September 10th, 1864; the emulsion used in the reported cases being made of beef-fat and the pancreatic juice of the pig The next report was published June 10th, 1865: the emulsion used in those cases being made of lard-oil, and the pancreatic juice of the pig. The last report was published November 11th and 18th, 1865; some of the cases having been treated with the lard-oil emulsion, and some with the emulsion of suet and pancreatic juice of the pig. The number of cases in the three reports is eighty-nine; but, out of these, only twenty were in the first stage, and in these the deposit of tubercle was considerable, and the general symptoms very marked; in fact, they were, as a rule, just passing into the second stage, before applying for treatment at the hospital. It is obvious, therefore, that, although the results were remarkably satisfactory, there was not in one of the cases the opportunity of trying the treatment in the most hopeful stage. In addition to this, I must remark, that for a long time, until I was satisfied of the good effects of the emulsion, I did not feel justified in giving it unless for some reason the patient refused to take cod-liver oil; so that this added another unfavourable feature to the cases selected for experiment. And, again, it is important to bear in mind that the emulsion, as we first prepared it, was necessarily in a much less satisfactory condition than the emulsions used in later experiments. From time to time, Mr. Heathorn introduced great improvements in the mode of manufacture, and these have been still further carried out by Mr. Schweitzer; so that the emulsions, as now sold by Messrs. Savory and Moore, are far more satisfactory in every respect than those which we employed for the hospital cases included in my reports.

Those accustomed to hospital practice will know well that the chances of treating tuberculosis in its simple and early stage, before the occurrence of tuberculisation, are very rare. Patients do not apply at hospitals for relief until they have some marked symptoms of disease, such as hemoptysis or purulent expectoration. When by chance they are seen in an earlier stage, they discontinue treatment directly the first feeling of debility is allayed. It is in private practice that the opportunity arises for detecting commencing tuberculosis; and it is especially within the province of the family doctor, frequenting the houses of his patients for other maladies, to keep a watch, especially in consumptive families, for the earliest dawn of impending tuberculosis.

In conclusion, I wish particularly to impress that, if the views here brought forward are correct, the importance of pancreatic emulsions of solid fat over cod-liver oil, in the treatment of tuberculosis, must be as great as that of supplying a plant with good soil instead of putting it into water. In the one case, a provision is made for the maintenance of normal life; in the other, death is only postponed by a temporary expedient.—*British Medical Journal, Jan.* 20, *and Feb.* 10, 1866, *pp.* 64, 139.

30.—NEW BI-VALVE SPECULUM FOR EXAMINING THE LARYNX, INVENTED AND PATENTED BY DR. LABORDETTE, OF LISIEUX.

[This instrument combines the functions of mirror and depressor of the tongue in one instrument, and a great deal of light is economised by reflection from the polished interior of the speculum tube.]

The description of the instrument given by the inventor is as follows:—

"The posterior blade, which is curved to fit closely the back of the mouth, descends more or less deeply into the pharynx; the inferior blade, which is shorter, penetrates as far as the base of the tongue, which it depresses by means of the lever spring fixed to the handle.

"The instrument, introduced into the mouth, is pushed as far back as possible; the posterior blade being in the pharynx serves as a point d'appui; the tongue is then immediately depressed in the manner before described by means of the lever-spring acting upon the anterior blade, and exposes the orifice of the larynx reflected in the mirror.

"There are two sizes, one for children and one for adults, which may be obtained, price 31s. each, through most chemists, or direct from S. Maw and Son, 11, Aldersgate-street, E.C."—*Medical Times and Gazette, Feb.* 24, 1866, *p.* 213.

31.—ON THE THERMOMETER AS A MEANS OF DIAGNOSIS BETWEEN TUBUCULAR AND NON-TUBERCULAR DISEASES.

By the Editor of the MEDICAL TIMES AND GAZETTE.

We again impress on our readers the fact that the temperature is always preternaturally raised when tubercle is being deposited in any of the organs of the body. This elevation often continues for many months. The temperature reached in tuberculosis varies from any point above that of health to 105° or 106° Fah. The usual maximum temperature of the day varies between 102° and 103° Fah.

Not unfrequently when tubercle is deposited in the meninges, or in the substance of the brain, the symptoms that result are almost indentical with those that occur from other diseases of that organ. In such cases by an appeal to the thermometer we often gain information by which we can learn the nature of the disease. The correctness of this statement is shown by the following case :—

The patient was a boy of 4 years of age ; previous to his admission into Hospital he had suffered from severe pains in the head, frequent sickness, and obstinate constipation of the bowels. There had been no delirium. He was ill rather more than five weeks, and gradually became comatose, from which he could only be partially roused. His head was thrown back, and was fixed in this position by the muscles of the back of the neck. The muscles at this part were rigid and contracted. There was ptosis with intermitting and fluctuating strabismus. The pupils were widely and irregularly dilated, and they acted but little under the influence of light ; the skin flushed up to a very unusual degree on irritation. The respiration and the pulse were very slow and irregular for a great part of the time. There was, however, no paralysis, nor rigidity, nor convulsive movements of the limbs. The coma deepened so that he could not be roused, and continued so till the time of his death. The lungs tested by physical signs were healthy. Such symptoms, occurring in a child 4 years of age, were sufficient to lead to the idea that the disease was tubercular meningitis. If the symptoms, however, be more carefully examined they will be found to be insufficient to establish such a diagnosis.

The head of the child was thrown back, and it remained in this position. To what was this position of the head due? In *Children* the three conditions that may cause this are :—1. Angular curvature of the spine. 2. Rickets. 3. Effusion into the ventricles of the brain. With rickets there is great muscular weakness, and the child has much difficulty in retaining the head in the erect position. The head being most weighted behind, therefore falls backwards ; also on account of muscular weakness the back is curved and the shoulders raised ; this is the common position of ricketty children. They are said to "sit all in a lump." If the position of the head mentioned above be due to this disease, this can be at once detected by the absence of the symptoms that accompany the other diseases producing it, and by the absence of any rigidity of the muscles of the back of the neck. If, moreover, the position of the child be altered, the head will roll forward, or to either side, and will assume the most dependent position. If it be due to angular curvature of the spine, this cause can be at once detected by inspection and by running the fingers down the spinal column.

If these two diseases can be excluded, and if the head be fixed back by the muscles of the back of the neck so that it cannot be pushed forward except with difficulty, and often not at all, and if the muscles of the back of the neck be rigid and firm, we are justified in diagnosing in a child effusion into the ventricles of the brain. This conclusion is based on many cases on which post-mortem examinations were made. In no case have we known the sign to prove incorrect.

In this case the head was firmly fixed backwards, and the muscles of the back of the neck were contracted and hard. There was no angular curvature of the spine. Hence we were justified in concluding that the child suffered from effusion into the ventricles of the brain.

Effusion into the ventricles is, moreover, sufficient to explain most of the remaining symptoms of the case. From this there results loss of consciousness, loss of sight, drooping of the upper eyelids, strabismus with irregularity of the pupils, flushing of the skin or irritation, slow and irregular pulse, the irregular breathing, and curving of the head backwards.

This case, and that which will shortly follow, prove that these symptoms are not significant of tubercle deposited in the meninges or in the brain. That they may result simply from effusion into the ventricles we conclude from a case in which these symptoms were present. At the post-mortem examination there was found considerable effusion into the ventricles produced by inflammation of the straight and one of the lateral sinuses, and the occlusion of these vessels by a firm, old, and adherent clot. This is the simplest form of the disease that can be met with, as it can hardly be maintained that the symptoms were caused by the inflammation of the sinuses, apart from their producing effusion into the ventricles. The brain itself, with the exception of the effusion into the ventricles, was healthy.

Nor can the remaining symptoms present in the case just given—namely, pain in the head, vomiting, constipation of the bowels—be accepted as proof of the tubercular or inflammatory nature of the affection of the brain; for such symptoms occur in other diseases, as this case proves. The utmost, therefore, that could be arrived at from the symptoms present was the certainty that the child had suffered from effusion into the ventricle of the brain.

What diseases may produce such an effusion into the ventricles of a child of this age? 1. Inflammation, simple or tubercular; 2. Tumours of the brain; 3. Coagulation of blood from inflammation of the straight or lateral sinuses, with occlusion, complete or partial, of these vessels. If other diseases cause the effusion, they are very rare. Of those mentioned, the former two are by far the most common. How are we to learn to which of these causes the effusion is due? The thermometer often gives us the information we require. We have seen that the deposition of tubercle causes an elevation of the temperature of the body. If, therefore, the temperature be normal, the disease producing the symptom is not tubercular. It may be objected that tubercle may be deposited in the brain, and the further deposition may cease, and that in such cases the temperature will be normal while the effusion remains, the symptoms resulting from this will also continue. It is true that such cases occur, but they are infinitely rare, as the deposition of tubercle very generally continues till the patient dies. This is rare of tubercle in the brain substance ; it is still rarer of tubercle in the meninges of the brain. How can we learn if the effusion is caused by non-tubercular inflammation of the meninges of the brain? If the temperature be normal at the commencement of the attack, this disease is excluded from the diagnosis. But children much more commonly survive the simple acute than the tubercular inflammation of the meninges of the brain. The non-tubercular inflammation of the meninges either destroys life in five or ten days, or the inflammation subsides and the temperature becomes normal ; but in such case the effusion into the ventricle continues and may increase in quantity. How are we to learn that in such a case the effusion is due to a previous inflammation? The two chief points to be relied on are, the age of the patient and the method of invasion of the attack. The former of these is of the most value. Simple inflammation of the meninges in children almost always occurs during the first few months of life. (Tubercle in any of the organs is uncommon under two years and very rare under one year of age). Non-tubercular inflammation of the meninges most commonly begins with an attack of convulsions—tubercular inflammation rarely so. The other cause also of ventricular effusion, namely, tumours, is very rare in children under two years of age. Thus the most common cause of such effusion in very young children is simple inflammation of the meninges.

But while the temperature of the body is raised both by tubercular and non-tubercular inflammation of the meninges in some part of their course, *the temperature of the body is normal in cases of non-tubercular tumours of the brain.*

In the case just given the temperature of the child was *normal*, and thus the nature of the disease was proved, and as the child came under observation early in the disease, we excluded simple inflammation of the meninges, and moreover the child was not of the age when this most frequently occurs, nor did it begin in the manner usual with this disease.

The diagnosis, therefore, was that the child suffered from effusion into the ventricles, and that this was most probably produced by a non-tubercular tumour (inflammation of the straight sinus and its occlusion by a clot is so extremely rare that this could with probability be excluded from the diagnosis). This conclusion proved correct, for on the post-mortem examination there was found a non-tubercular tumour of the size of a Maltese orange, situated in the anterior and inferior part of the cerebellum. This had pressed on the right hallf of the medulla oblongata, and had caused much atrophy of this part.

We venture to add another case to exemplify the use of the thermometer in similar cases to the one just given. A boy, aged 11, had suffered from severe pain in the head, with vomiting and obstinate constipation of the bowels. When first seen these symptoms still continued. His head was slightly thrown back, and the muscles at the back of the neck were rigid. His face was flushed, and he was partially unconscious. The pupils were dilated, and failed to act under the influence of light. There was slight strabismus, but no paralysis nor rigidity of any of the limbs. These symptoms are those most commonly met with in tubercular meningitis. *His temperature, however, was always normal.* Thus the disease could not be of a tubercular or inflammatory nature. It was considered probable that he suffered from a non-tubercular tumour of the brain. The symptoms described above disappeared in a few days. He was frequently seized with similar attacks, from each of which he recovered. He died suddenly from some unexplained cause. There was found a large fibro-plastic tumour, situated in the cerebellum. The ventricles were dilated, and contained about 3 oz. of fluid.

These cases serve to illustrate the assistance we may derive from the thermometer in cases similar to those described.

If, in such cases, the temperature be normal, the disease is not tubercular ; if, on the other hand, the temperature be elevated in a child over two years of age, it is in the highest degree probable that tubercle is being deposited in the meninges or in the brain of the patient. For if the elevation of the temperature should be caused by some intercurrent disease, such disease can mostly be detected.

Before concluding this article we add a few words concerning effusion into the ventricles of the brain, and the symptoms that result from this.

If the effusion occur quickly, the consciousness is greatly lessened ; if it occurs more slowly, the consciousness is but little impaired. The loss of consciousness is caused by the pressure of the grey matter resulting from the effusion. The brain, however, accommodates itself to slow pressure, and thus the consciousness is but little altered when the effusion occurs gradually. Again, when the effusion has occurred quickly, and the consciousness has been quite lost, if no further effusion take place, or if its further increase be slow, the intelligence of the child to a great extent returns, and after some time has elapsed, it may be chiefly restored. In most of the cases similar to those described, it is the rapid effusion by producing loss of consciousness that quickly destroys life. When the effusion is slowly produced, the children may survive for years, and in some cases they live the number of years usually allotted to man.

The rigidity of the muscles at the back of the neck, and the degree to which the head is drawn back, is proportionate to the amount and rapidity of the effusion. If this displacement of the head is but slight, and but a short time has elapsed, the amount of the effusion is small; if, on the other hand, the effusion has occurred slowly, and the head has had time to enlarge, then this rigidity of the muscles, and this position of the head, is but slightly marked. This symptom also disappears after the effusion has lasted some time, if no further increase occurs. It disappears much earlier in young people, in whom the bones of the head are ununited, than in those children whose skull is firmly closed. In the latter it lasts often for a considerable time; it continues long after the intelligence of the child has greatly returned.

The ptosis, strabismus, and inequality of the pupils are but slightly marked when the effusion is slowly produced, and this soon disappears when the further effusion of fluid into the ventricle ceases. Loss of sight, however, is mostly present whether the effusion occurs quickly or slowly, and is one of the last and rarest symptoms to disappear.

The shape of the head, when the fontanelles are open, is so peculiar and characteristic that we need not dwell on it here. The shape, however, somewhat differs when the bones are firmly closed. In such a case, when the effusion occurs slowly, and consequently the patient lives some months, the enlargement is most marked at that part of the skull occupied by the parietal bones. The frontal bone is comparatively little affected. Thus there is formed a very prominent and acutely formed ridge, corresponding to the junction between the parietal and frontal bones. The sudden elevation in some cases amounts to half an inch in extent. It occurs earliest, and is most marked, in the temporal regions.—*Med. Times and Gaz., March* 24, 1866, *p.* 311.

32.—ON A CERTAIN FORM OF HEMOPTYSIS UNASSOCIATED WITH PULMONARY TUBERCULOSIS.

By Dr. Richard Payne Cotton, Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

[That hemoptysis is met with in a very considerable number of non-tubercular cases is well known; its occurrence however always suggests the presence of tubercle in the lung. There are many conditions which may give rise to hemoptysis.]

In the present instance I am anxious only to draw attention to a not unfrequent, but, so far as I know, little recognised form of non-tubercular hemoptysis, met with chiefly in the female sex, but sometimes also amongst males, generally in the early period of life.

It may simplify my description of this variety of hemoptysis if I give a brief account of two or three cases in point.

A young lady, aged 18, recently arrived from a residence in one of the West India islands, was supposed to be phthisical. I was requested to see her, and report upon the nature of her disease, about which several very conflicting opinions had already been given. She was anæmic, nervous, and out of health; had a dry cough, but had not become thinner; her catamenia were regular, but scanty; her appetite was capricious; and she had had frequent hemoptysis, which there was every reason to believe did not proceed from either the mouth or fauces. The blood, upon examinatiom was found to be thin and watery, of a dark colour, free from coagulum, unassociated with either bronchial or salivary secretion, and in general appearance much resembling a mixture of *red-currant jelly and water.* I was informed that this was its general character. Sometimes it had been considerable—as much as half a pint in twenty-four hours; at others, it would not exceed a teaspoonful or two during the same period; sometimes it would be scarcely enough to tinge a pocket-handkerchief, and often it would disappear for days together. This state of things had existed for nearly two years, causing great anxiety to the patient and her friends, from a belief that it was indicative of pulmonary disease. Careful examination of the chest, however, failed to elicit any evidence that such was the case. Rest, change of air, and tincture of sesquichloride of iron, entirely restored this patient to health. It is now more than three years since I was consulted, and I heard a short time back that the young lady was in perfect health.

A case very similar to this came under my notice two years ago in consultation with Mr. Humpage, of Upper Seymour-street. A young lady, aged 24, had long been delicate, and was supposed by her family to be consumptive. She had become thinner; had had a dry cough for some length of time; and had spat blood. Upon examination, this was found to present an appearance very similar to that described in the preceding case; it looked, in fact, more like watery red-currant jelly than anything else. As in the other patient, there were no decided physical signs of tubercular disease; and we came to the conclusion that the patient was not phthisical. Time has justified our diagnosis; Mr. Humpage having lately informed me that the young lady had, for some months, lost the symptom which had caused so much alarm, and was in good health.

Another case, which I shall even more briefly relate, came under my notice about eighteen months back. It was that of a young lady, 12 years of age, of slender and somewhat delicate appearance, but free from every symptom of tubercular affection. Mr. J. N. Winter, of Montpelier-road, Brighton, frequently saw this patient, and quite agreed with me as to the nature of her disease. She first alarmed her parents by spitting up, just after going to bed one night, a considerable quantity of blood. This was found upon examination to be watery and dark-coloured—in fact, of the *thin red-currant jelly character* already described. This symptom has recurred at various intervals with more or less intensity, and the child still remains delicate, but without any indications of tubercular disease. At the time of her attacks her tonsils and pharynx are somewhat congested, her gums spongy, and her fingers even have sometimes exuded a little watery blood, just as one sometimes sees in extreme cases of purpura. It is evident that, in this instance, the blood escapes not only from the mucous membrane of the respiratory passages, but also from that of the throat, tonsils, and gums.

Other cases of this form of hemoptysis have fallen under my observation; but I shall not specially refer to them in consequence of not knowing their sequel. At least twelve or thirteen have happened in my own wards in the Hospital for Consumption. Two only of these were males; the rest were females, generally of delicate and nervous appearance, and under the age of 30. Several had very suspicious symptoms of phthisis; but the physical signs failed to exhibit any evidence of pulmonary tuberculosis, and most of them improved in health under appropriate treatment. In every case the expectoration was of the same general character; and sometimes it was mixed more or less with bronchial mucus, slightly tinged perhaps with blood, and sometimes with salivary secretion; but more frequently it was simply watery blood, resembling, as I have described, a mixture of red-currant jelly with water.

The following are the conclusions at which I have arrived from a consideration of the preceding notes:—

1st. There is a form of true hemoptysis in which the expectoration is of a dark colour, and of a more or less watery consistence, bearing a close resemblance to a mixture of red-currant jelly and water.

2nd. That such hemoptysis is of non-tubercular origin, and may proceed from any part of the gastro-pulmonary mucous membrane.

3rd. That it is attributable to a morbid and fluid condition of the blood, allied, at least in appearance, to that which is met with in purpura and scurvy.—*Lancet, Dec.* 2, 1865, *p.* 618.

33.—ON THE USE OF PHENIC (CARBOLIC) ACID FOR THE CURE OF PHTHISIS.

By Dr. J. R. Wolfe, Aberdeen.

Some two years ago the eminent physiologist, Prof. Longet, who had an attack of hemoptysis with tuberculosis, made some experiments upon himself with phenic (carbolic) acid, by which he was greatly benefited. It is owing to the favourable report of that distinguished patient that this substance is now largely employed by some of the French physicians. Dr. Labori, of the Convalescent Hospital (L'Asile Imperiale de Vincennes), has told me that he administered it to between 200 and 300 patients in different stages of phthisis, with most favourable results. The mode of administration is as follows: fifteen drops of the pure acid are dissolved in ʒij. of spirits, and the solution mixed with ℥xxxij. of water. This quantity is administered daily, partly by the stomach and partly by the inhalation of the fluid in a pulverised state.

There are several instruments now in use for reducing fluid to fine molecules.

The internal administration of creosote and the inhalation of tar vapour have long been recognised as useful remedies in phthisis; but, considering the *modus administrandi* just referred to, it is not quite as if we wanted to introduce an old friend with a new face. The great objection to the inhalation of tar and terebinthinate vapour was, that in order to be effectual it must be persevered with, sometimes for years. Even in the case of that valuable and standard remedy, cod-liver oil, when first introduced for the cure of phthisis, some questioned its remedial effects simply for want of perseverance to give it a long enough trial. Now, by means of the pulverising method, the larynx, trachea, and, as has been proved upon animals, the lung tissue, are brought into direct contact with the remedial agent, which is also absorbed into the system, and it has besides the advantage that the patient can regulate the quantity to be inhaled.

As an adjuvant to cod-liver oil, it is beneficial in all stages of phthisis; but it is eminently useful in arresting hemoptysis, in allaying irritation, and arresting the profuse secretion in cases of chronic bronchitis and of cavities in the lungs, of laryngeal phthisis, and of colliquative sweats, and I think it deserves a trial in cases of diphtheria. I would here take the opportunity of strongly recommending the use of the pulveriser. From what I have seen of its efficacy for the last eight years in laryngeal, bronchial, and lung affections, in which various medicines have been administered in that form, I am confident that it needs only to be known to the profession in order to be generally employed. It opens a new field of therapeutics, and enables us to cope with disease which the gargle, probang, and even the ordinary inhaler could not reach.

The pure phenic acid directly applied is used in bites of the charbon fly or any poisoned wounds. Charpie dipped in a solution of ʒss. of the acid, in ℥j. of spirits, diluted with ℥xv. of water, is used by Maissonneuve and Gosselin for dressing indolent ulcers and suppurating surfaces. A similar dressing answers excellently in open cancer and fœtid discharges of all kinds. The only objection to its use, is its strong and rather disagreeable smell. To obviate this, I have made some experiments, and find that cotton-wool saturated in the pure acid, and then pressed to get rid of the excess of acid, and dried, leaves enough of the acid to act upon the wound without leaving so much as to make the smell disagreeable. The cotton so prepared is kept in a tin box to prevent volatilisation of the acid.

As far as my experiments have gone, I am satisfied with its beneficial results. It causes a healthy granulating surface, counteracting the tendency to hyperplasie and suppuration. But my study of the subject is not sufficiently advanced to enable me to enter into particulars in this communication. I may express it, however, as my opinion, that its employment for dressing wounds will greatly lessen the risk of purulent and erysipelatous infection in our Surgical wards.—*Med. Times and Gazette, Nov.* 25, 1865, *p.* 572.

DISEASES OF THE ORGANS OF DIGESTION.

34.—CONCERNING THE TREATMENT OF CONSTIPATION AND OF STOPPAGE OF THE BOWELS—WITH SPECIAL REFERENCE TO THE USE OF ATROPIA AND OF GALVANISM.

By Dr. ALEXANDER FLEMING, Senior Physician to the Queen's Hospital, Birmingham.

[Dr. Fleming has now for many years employed atropia in cases of epilepsy, asthma, and other disorders; and about eight years since his attention was forcibly directed to its effect in promoting the action of the bowels.]

In the course of my practice, when I had occasion to prescribe atropia, I noticed frequently that, in from one to four days, a slight relaxation of the bowels took place. The stools were but little altered in character, and the intestinal secretion but slightly increased; still, the action of the bowels was decidedly easier, and, if constipation had existed, it was removed. Occasionally the purging was more marked. I believe that this effect is brought about by *increased* peristaltic action. The cause of this increased action may be direct stimulation of the muscular coat by the atropia carried to it with the blood; but other causes have been suggested to my mind from close observation of the effects of atropia on other parts of the body, more especially on the throat, stomach, and bladder. When this drug is exhibited in small and medicinal doses, it causes remarkable dryness of the throat and tongue, difficulty in, yet constant efforts at, swallowing. The changes in the act of micturition are remarkable and noteworthy. This is often hurried and frequent, sometimes interrupted, and occasionally there is slight strangury. I have seen a patient compelled to make water every five minutes. In the throat, the mucous secretion is obviously checked, the membrane is seen to be dry, and the surface is thus rendered more susceptible of irritation; hence the constant efforts of deglutition. I believe the effect of the drug on other mucous membranes to be of the same nature; and, in the bladder, this arrest of the mucous secretion results in irregular and frequent micturition. According to this view, its action on the bowels is easily explained. The mucous secretion being checked, the irritation caused by the contents of the intestinal canal, when its surface is thus unprotected, provokes more prompt and vigorous contractile action.

Secondly, atropia constricts the smaller arteries; and we can understand that a gut, dormant and paralysed by distension, is the subject of passive congestion, the continuance of which will contribute to maintain its state of inertia. Atropia, acting on the arteries, checks the supply of blood to the bowel, relieves the congested muscle, and thus facilitates its return to healthy action. This *modus operandi* is analogous to the well-known effect of blood-letting or leeches, in relieving the congestion of, and unloading, an inflamed intestine.

Accepting these views of the *mode of action* of atropia on the bowels, we at once perceive its advantage over the ordinary irritant purges in the treatment, not only of simple constipation, but especially of the more serious and alarming cases of intestinal obstruction from impacted fæces. The ordinary irritant purges provoke increased secretion and peristaltic action of the gut *above* the obstruction; this may succeed in propelling the accumulation forward, but should it fail in doing so, we have inverted action and vomiting, with the further risks of enteritis, and general, and it may be fatal, exhaustion of the patient. Atropia, on the contrary, operates through the blood on the entire canal; acts directly on that part of the gut which is distended by the accumulation, and so paralysed. Deprived, by the drying qualities of the drug, of its natural coating of mucus, the mass more readily provokes irritation; the natural contractile action is re-established; and the bowel is more or less quickly relieved of its contents.

There is another circumstance connected with atropia, which distinguishes its operation from that of common purgatives; its action is not followed by reaction; its relaxing power is not succeeded by a disposition to constipation. On the contrary, the improved action of the bowels is, comparatively speaking, sustained.

The powder and extract of the crude drug belladonna have been employed successfully in constipation by Bretonneau, Trousseau, Fleury, Drs. Brinton, Routh, Fuller, Leared, and others; and a most interesting paper on the Use of Belladonna in Intestinal Obstruction was read at the Bristol meeting of our Association in 1863.

The causes of constipation, and the conditions under which it occurs, vary infinitely; each case requiring separate consideration, especially as regards the hygienic and dietetic treatment.

It is not my intention to enter now into the question of the regimen and diet of constipation; but, to prevent any misconception as to my view of their value, let me state distinctly, that I assign to them the first place in importance as curative means, and regard medicinal agents in the light only of valuable auxiliaries. At the same time, the error should be avoided of underrating the value of medicines. In former times, they were relied on too exclusively; of late, under the influence of hydropathy and homœopathy, their importance has been most unwisely neglected. It is the function of the philosophical therapeutist to recognise the respective value of all remedial agents, whether hygienic, dietetic, or medicinal, and to assign to each their relative importance.

This truth is forcibly illustrated in the disease now under consideration; for while, in the more simple forms of constipation, regimen and diet are often equal to the cure, many examples of a more obstinate nature have occurred to me, where these means alone have signally failed to give relief, but where recovery has ensued when the treatment I shall now describe has been strictly followed.

In cases of *simple constipation* I have exhibited atropia in various forms, both in pill and in solution; but my later experience has led me to the adoption of a plan of treatment, of which the following is an outline.

The subjoined draught is given the first thing in the morning and the last thing at night on an empty stomach.

℞. Magnesiæ sulphatis ʒj ; acidi sulphurici aromat. ♏x ; tinct. aurantii ʒj ; aquæ ad ℥j. M.

Ten minims (containing one-sixtieth of a grain) of a solution* of atropia are added to the draught at bed time; and the dose is increased nightly by two minims, until a very slight degree of the earlier physiological effects of the drug—dry throat, wide pupil, and dim sight—is produced. This is attained with much precision and safety; but it may be necessary to give thirty, forty, or even fifty minims, according to the strength of the patient, before this result is attained. The dose should then be somewhat diminished, and continued at the reduced quantity for two or three weeks, as circumstances may indicate. I then discontinue the drug gradually; and finally replace it with strychnia, giving five minims of a solution† in both morning and evening draughts for a week or two; or the strychnia may be given alone as soon as the saline draught can be dispensed with. This commonly suffices to restore the normal tone of the bowel, and completes the medicinal treatment.

When constipation is neglected, the fæces accumulating gradually distend the bowel, and finally deprive the muscular coat of its irritability and contractility, and we have established one of the most frequent forms of *obstruction* of the bowels. (The observations in the present paper refer to this form only.)

If, after a moderate use of the ordinary purgatives by the mouth and in the form of enemata, the obstruction shows no disposition to yield, and the patient suffers from pain and distension of the belly, with (it may be) nausea and vomiting, I prohibit entirely the use of the more powerful cathartics, the exhibition of which increases the vomiting and irritation and may provoke inflammation. I desire the patient to be confined to liquid food; viz., milk and beef-tea alternately every four hours. If there be much vomiting, I direct the milk to be mixed with one-third of Carrara water, and the quantity of food at each meal to be very small, until the irritability of the stomach has subsided. The following draught is prescribed every four hours before each meal.

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* The solution of atropia which I use is made thus:—Atropia, 1 grain; distilled water, 5 drachms; Dissolve *thoroughly* with the aid of a few drops of diluted muriatic acid, and add of rectified spirit sufficient to make ten drachms. This solution keeps well, and is of uniform strength. The tincture and extract of belladonna, however carefully prepared, vary much in power. I have found the tincture of one chemist seven times the strength of the same preparation from another and equally respectable chemist; and the extract is even more uncertain. The internal, and at the same time efficient, use of these preparations is for this reason very unsafe. The solution is so proportioned, that ten minims, containing one-sixtieth of a grain of atropia, is the commencing dose for the adult.

† The solution of strychnia which I use is made thus:—Strychnia, 2 grains; distilled water, 5 drachms. Dissolve the strychnia *thoroughly* with help of a little diluted muriatic acid, and add of rectified spirit sufficient to make ten drachms. This solution has the same advantages over the powder, extract, and tincture of nux vomica, that the solution of atropia has over the tincture and extract of belladonna. It is uniform in strength, passes readily into the circulation, and the dose can be apportioned with accuracy. The ordinary commencing daily dose is ten minims, and contains one-thirtieth of a grain of strychnia.

> ℞. Magnesiæ sulphatis ʒj; solutionis atropiæ (author) ♏ iv; acidi sulphurici diluti ♏x; aquæ, ad ℥j. M. Fiat haustus.

Should there be much spasmodic pain, I add half a drachm of chloric ether, prepared by distillation, to each dose. This draught is, for the most part, readily borne by the stomach; promotes gently the action of the bowels; and softens their contents. The atropia favours, in the manner already indicated, the contractile power of the gut. In using atropia in the manner specified, the patient *must* be seen twice daily; for, as a slight degree of the physiological action of the drug should be induced, the dose should be increased, diminished, or omitted, according to the effect observed. If pain and indications of approaching inflammation be present, warm fomentations to the belly are demanded; on the other hand, if these symptoms be absent, the general purpose of the medication is promoted by frictions two or three times a day with warm liniments; the rubbing to be so applied as to promote the normal course of the intestinal contents.

In a considerable proportion of cases, this treatment alone affords the desired relief; in other and more obstinate examples, we have to conjoin the use of aperient enemata. These should be used two or three times daily; and be introduced by means of the rectum-tube as high as possible into the bowel. When ordinary injections fail, ice-cold water sometimes succeeds; and it is well to bear this in mind.

In inserting the tube for any distance into the rectum, much annoyance is often experienced by its doubling upon itself. I have overcome this difficulty by the use of a stilette within the tube. Should any impediment, such as a fold of membrane, obstruct the passage, the withdrawal of the stilette for an inch or two allows the flexible tube to adapt itself to sinuosities of the intestine, and facilitates its further introduction. I have had the tube marked off in inches, to indicate precisely the extent of insertion. The higher it can be passed with safety, the more efficient is the injection.

Between the enemata, galvanism should be applied to the bowels. This agent should be conducted through the rectum, and passed, as nearly as may be possible, through the paralysed gut; care being taken to employ it gently, but repeatedly, our object being rather to restore the action of the bowel by small but successive doses of the stimulus, instead of attempting to dislodge the impacted contents by one powerful application of the galvanic current.

Employed in this manner, the galvanism is infinitely more efficient than when passed in the ordinary mode from the back to the belly. It gives rise, however, to acute suffering; and, unless it be used gently, as I have directed, tends to exhaust the patient.

The following is the apparatus I employ. Through a rectum-tube, twenty inches long, a copper wire is passed with a brass button at the distal extremity; a hole being drilled at the opposite end for the reception of one of the wires of the galvano-magnetic machine. The tube being passed into the rectum, the current is completed by applying to the abdominal walls the excitor of the other wire. This excitor—which is sometimes a sponge, sometimes a cylinder of metal, and at other times an olive-shaped metallic button covered with leather—should be moved along the course of the distended bowel, and its position constantly shifted; the skin of the belly being kept moist with salt water.

When the current is gentle, its application may be continued for half an hour at a time, and from time to time a sharper shock may be transmitted; the force of the current, and its duration, being carefully adapted to the strength of the patient.

I use Duchenne's galvano-magnetic machine, as with it the current can be usefully modified in various ways.

I have employed galvanism by the rectum in the manner indicated, in intestinal obstruction, since 1857.

Such is the outline of the treatment of intestinal obstruction from impacted fæces, which I can say with confidence has furnished very satisfactory results.

In conclusion, it may not be superfluous to add that, in the diagnosis of the cause of stoppage of the bowels, we may often be at fault and treat as a case of simple accumulation, some one or other of the many forms of insuperable obstruction. It is satisfactory to know, that the medicines here recommended may do good and can do no harm in such a case; but the suspicion of error in diagnosis obviously enjoins much caution in the use of galvanism. This was well shown in a case of complete obstruction in an elderly female, which I saw in consultation with Mr. Hoskins of this town. The usual treatment had been judiciously employed. Atropia was then administered, and most efficiently, so far as regards its physiological action. Galvanism was also carefully used. The obstruction was insuperable, and finally proved fatal. The patient, however, lived for a week after the use of these measures, in comparative ease;

showing that no harm had resulted from their employment. A *post mortem* examination was not obtained.

For the better illustration of the nature of the results furnished by the treatment described above, I subjoin histories in detail of five cases.

Case 1.—Severe Constipation of three years' standing; Colic, Vomiting, and serious Exhaustion; Failure of ordinary Methods of Treatment; Use of Atropia; Recovery.—The patient, a young lady, aged 18, came under my care in September, 1861. She was then suffering from almost constant spasms of the bowels, causing intense agony, aggravated after every attempt to take the simplest food; the vomiting and retching were incessant. There was no fever nor tenderness of the belly. She was very weak, confined to bed; the countenance was pale; the pulse was feeble, but not frequent; and the case wore an aspect of much anxiety.

History.—Three years previously to the time of my seeing this patient, she had suffered from prolonged and most obstinate constipation, brought on by gross neglect of the bowels at school. The stomach became irritable, rejecting many articles of food; and the least imprudence in diet brought on spasmodic attacks of pain in the belly and vomiting. After repeated, but unsuccessful, attempts to obtain relief under medical treatment in various places, her friends placed her at Malvern under the water-cure. She derived no permanent benefit; though, from the strict attention to diet, her general health was improved, and the attacks for a time were not quite so frequent. They returned, however, shortly with redoubled violence, accompanied by excessive flatulence, twisting of the bowels, and severe suffering. The irritation of the stomach increased, until the simplest food produced so much pain that she was, as she expressed it, "terrified to eat."

After a careful examination of the case, in which I had the valuable assistance of Professor Simpson of Edinburgh, it was concluded that there was no inflammation, but that the peristaltic action of the bowels was perverted; in the smaller intestines it was abnormally active, leading to severe spasmodic contortions and heaving of the belly; while the colon was unduly distended and more or less completely paralysed. There was then no absolute obstruction, but a constant tendency to constipation.

Acting on this diagnosis, the treatment of constipation described in this paper was adopted. The patient was restricted to a milk-diet. Atropism was fully induced; blisters were repeatedly applied to the belly; and aperient enemata were administered from time to time. Under this medication, which occupied about a fortnight, the colon was gradually unloaded of its accumulated contents, and the vomiting and spasms quickly subsided. The stomach became tolerant of food; the normal appetite returned; and the patient rapidly regained flesh and strength.

The cure was absolutely permanent. The stomach was delicate for some months; but, with a moderate attention to diet, the patient continued well. There was no return of constipation or spasms of the bowels. The young lady is now in the enjoyment of excellent health in all respects.

I have no hesitation in describing this as a remarkable case. The relation between the physiological action of atropia and the improvement of the symptoms was unequivocal; lastly, the short duration of the treatment, and the completeness of the cure, contrasted very strikingly with the lengthened and unsuccessful efforts at relief which had been made at previous times in the earlier history of the case.

Case 2.—Stoppage of the Bowels from Fœcal Accumulation; Colic; Stercoraceous Vomiting; Failure of Ordinary Treatment; Atropism on the thirteenth day; Recovery.—(This case occurred in the practice of Dr. Spencer Thomson of Burton-on-Trent, to whom I am indebted for the following notes.) L. R., aged 45, married, a thin, spare, delicate woman, was seized on the night of October 20th, 1863, with severe twisting pain in the bowels, incessant vomiting, and complete constipation. The bowels had been opened on the previous day. Within twenty-four hours the vomiting became stercoraceous; and continued so, with intermissions, for thirteen days of the sixteen during which the obstruction lasted. During the latter half of the period of the attack, there was much tympanitic distension and tenderness, but there never were any positive signs of peritoneal inflammation.

The remedies resorted to were at first bismuth, prussic acid, opium, and chloroform, to subdue the irritable condition of the stomach; counter-irritants to the belly; and a persevering use of every variety of purgative injection.

No relief was obtained. On the thirteenth day of the attack, one drachm of the following mixture was directed to be taken every hour.

℞. Solutionis atropiæ (Fleming) ♏xv; acidi hydrocyanici diluti ♏xv; aquæ ℥ij. M.

Soon after the above was commenced, the stomach became quiet, and retained the medicine, as well as small quantities of beef-tea and brandy and water. In forty-eight hours, the atropism being now manifest, the obstruction showed signs of yielding, and the bowels were gradually completely relieved; and, on the sixteenth day from the commencement of the attack, all signs of obstruction were removed. Mrs. R., gradually recovered, and is now (April 1864) in her usual health, having had no return or even threatening of the disease since. The catamenia, which were irregular for some months previous to the attack, have since been quite normal.

Case 3.—Obstruction from Fæcal Masses; Constant Sickness and Vomiting; Use of Salines, Atropia, and Galvanism; Gradual breaking down and Removal of the Accumulations; Recovery.—S. L., aged 50, when placed under my care, was suffering from severe tormina, constant retching and vomiting, and obstruction of seven days' standing. The pulse was frequent and feeble, the countenance anxious, and the skin covered with perspiration. Three distinct tumours were to be felt in the belly; one in the upper part of the ascending colon; one at the corner, between the ascending and transverse colons; and the third in the transverse colon itself. These were moveable, and presented the other features of fæcal masses. They differed in hardness, the central one being much the hardest of the three. The diet, saline draughts, atropia, aperient injections with the rectum-tube, and after several days galvanism, were employed as indicated in my paper. The vomiting rapidly ceased, and the patient was soon able to take food well. In three days, the bowels began to act, and continued to do so two or three times daily; the evacuations were small in quantity and fluid. The tumours in the belly gradually decreased in size, and finally were dispersed.

In the discharges, the gritty and insoluble components of the fæculent concretions were readily distinguished; they were composed of small pieces of bone, undigested tendon, &c. The patient was an old dyspeptic, through whose stomach, the harder portions of the food were apt to pass undissolved.

Galvanism in this case was used daily, but very gently, for a fortnight, and was passed through the colon in the neighbourhood of the swellings. When the galvanic current was powerful, the suffering was acute, and could not have been maintained without risk of undue exhaustion. Therefore the utmost care was required in its administration; and no attempt was made to secure the immediate expulsion of the fæcal masses.

In the third week of the treatment, the intestinal canal was clear of obstruction; and the patient, though very weak, was convalescent. He made a good recovery.

Case 4.—Simple Constipation of long standing, in a Healthy

Subject; Failure of Dietetic and Ordinary Modes of Cure; Use of Atropia; Recovery.—J. J., aged 27, a strong healthy young man, living in the country, applied to me in April, 1862, on account of constipation. His bowels had for years been obstinate, and for eighteen months he was obliged to take aperient medicines daily; otherwise he would pass three or four days without going to the closet, and would then suffer much pain at stool. So long as he continued taking aperients, his appetite and general health were good. He had several times been under medical care, with temporary advantage; and had endeavoured to rectify the evil by diet and exercise, but unsuccessfully.

I directed him to sponge with salt water once daily, in the morning; to rub the belly vigorously; to take abundant exercise (without fatigue); to omit from the diet, tea, coffee, and stimulants, with the exception of a glass and a half of claret mixed with water to dinner; to take cocoa to breakfast, porridge to supper, and vegetables and fruit in moderation. The medicinal part of the treatment consisted of the saline draughts atropism was induced. The improvement was slow, but very and atropia, as indicated in this paper. A moderate degree of marked. In three weeks he was able to discontinue his medicines. but has ever since persevered more or less closely with the regimen and diet. It is now eighteen months ago; and he has not during that time, except at rare intervals, been troubled with constipation.

Case 5.—*Obstruction; Slight Stricture; Fœcal Accumulation; Severe Tormina, Sickness and Vomiting; Salines, Atropia, and Enemata; Recovery.*—(I saw this patient, whose case I will relate very briefly, in October of last year, in consultation with Mr. Ross Jordan.) W. C., aged 35, had suffered for five days from obstruction, severe tormina, and incessant retching and vomiting. There were tenderness, distension, with dull percussion in the left iliac region. There was obvious accumulation in the descending colon. Salines, atropia, and enemata. were employed, with the result of gradually unloading the bowel, and restoring the healthy character of the evacuations, with the exception that the stools continued to be slightly flattened.

It appears that a year ago he had sought advice, on account of indigestion, from a quack, who gave him four doses of some drug (probably lobelia), which purged him very severely, causing much pain, and discharge of blood and mucus. Since then, the bowels have continued to trouble him; and the symptoms point to a slight constriction—the consequence, probably, of inflammation—about the sigmoid flexure.—*British Medical Journal, Dec.* 1865, *p.* 651.

35.—ON THE TREATMENT OF DIPHTHERIA WITH HYPOSULPHITE OF SODA.

By J. Clarkson Maynard, Esq., Wisbeach.

[The plan of treatment pursued in a case of diphtheria by Mr. Maynard, and Dr. Tubbs, of Upwell (to whom the idea of using hyposulphites was first suggested by his gardener arresting vine disease by means of sulphur), is as follows.]

On first visiting a case, if not very far advanced, and in which only a few spots are visible, the throat is dressed twice a-day out with a strong solution of the hyposulphite of soda—viz., ʒiij. of the hyposulphite, glycerine ʒij., with ʒvj. of water. This generally removes the incipient exudation in forty-eight hours, sometimes in less. But if the case is an advanced one and the parasitic plant is making rapid strides, we wash the throat well out with warm water by means of one of Maw's flexible syringes. This is alike agreeable and most beneficial to the patient. The affected parts are then dressed with the strong solution, and a gargle of ʒss. of the hyposulphite to half a pint of water, with ℥ss. of glycerine, is given, to be used every hour.

The effect of the solution upon the exudation is most marked. It appears to solidify and dry up the false membrane, and when the syringe is again used, which is to be frequently done, the force of water will, if not completely, nearly entirely wash it away. The exudation in this way seldom or ever re-forms, and the patient makes comparatively a rapid recovery. In cases of a graver character, and where there is a larger collection than usual of inspissated mucus, we clear out the posterior nares by means of a powerful curved leaden syringe which is introduced into the nostril. In the putrid stage, and when the unpleasant odour from the throat is very offensive, a small quantity of Condy's disinfecting fluid added to the water with which we syringe the part has proved of great advantage. I may add that from half a gallon to a gallon of warm water ought, certainly in bad cases, to be thrown into the throat three or four times a-day. The ext. belladonnæ applied externaly has proved very useful where there has been much swelling.

In cases of very young children where it is difficult to dress and get at the throat, we give the hyposulphite internally, from gr. j. to gr. iij. every four hours, and allow them to swallow the gargle, which, by the way, they very frequently do without permission. Dr. Tubbs informs me he is now giving to adults gr. viij. every four hours. Port wine, beef-tea, brandy, and bark are, of course, given in suitable quantities, and in cases where there was much prostration we have occasionally thrown up, with very satisfactory results, an enema of port wine, beef-tea, and isinglass.—*Med. Times and Gazette, Dec.* 30, 1865, *p.* 714.

36.—TRACHEOTOMY IN CROUP AND DIPHTHERIA.

By Dr. George Buchanan, A.M., Surgeon to the Glasgow Royal Infirmary.

[Dr. Buchanan considers that tracheotomy is a most important means of saving life in the later stages of croup and diphtheria if performed before the symptoms of diphtheria have been allowed to wear out the patient.]

The following five cases bring the number of my operations up to twenty-six, with the result of nine cures, a proportion sufficiently encouraging, as compared with the published statistics of other operators.

Case 22.—John G., aged six years, had been ailing three days before Dr. M'Gill was called to see him, on the 27th January, 1865. At that time he had symptoms of croup, for which suitable treatment was ordered. He was relieved next morning, but at night got worse, and continued to do so till the evening of the 30th, when I was called to see him. It was quite evident that the tracheal effusion was considerable, as the dyspnœa was urgent; and medical treatment having failed, I proceeded to operate. As usual, the immediate effect of the operation was complete relief to the breathing. He passed a quiet night and next morning was breathing easily. I was, however, uneasy to find that but little secretion passed by the tube, and there was a tendency to dryness in the tracheal membrane, as evinced by a whistling sound. This is a most unfavourable symptom, indicating an extension of the inflammation. A copious secretion, even becoming muco-purulent, necessitating removal of the inner tube very frequently, is a far more favourable condition than that described above. I always feel anxious when the dry whistling sound is present. The tube was frequently cleaned out with a moistened feather; steam was made to pass freely over the patient's bed, but to no purpose. I was not surprised to find breathing becoming more laborious, the strength sinking, and death taking place on the 1st February.

Case 23.—On the 22nd of June, Dr. Gray requested me to visit —— Fitch, aged five years, who had been suffering from symptoms of diphtheric croup for a week. On the morning of my visit dyspnœa had become urgent, and was increasing in severity. Dr. Gray's previous experience of the disease had led him to conclude that it had now passed beyond the influence of medicine, and as soon as he formed that opinion he called for my aid. The child was in great distress, and I recommended that the operation should be performed without delay. In this the parents at once acquiesced. The opening of the trachea and insertion of the tube afforded immediate relief, and the progress towards recovery was rapid and uninterrupted. Messrs. Hollings and Gray, two of my students, took charge of the tube for the first two nights, and their reports of the child's condition from the time of operation were most satisfactory. The tube was allowed to remain for ten days, as it caused no annoyance, and the secretion soon became so thin that the effort to cough cleared the tube without much manipulation after the second day. In this, as in other cases, it was observed that milk sometimes ran out of the wound when the patient began to drink; but this was evidently from some of the fluid getting through the glottis, owing to the loss of sensibility or power which often accompanies this affection. A few days after the tube was removed the wound was covered with granulations, and soon completely healed.

Case 24.—This was one of the most interesting and instructive cases I have had. The little patient lived for thirteen days, and the cause of death was inflammation and effusion setting in further down the air passages, after he had recovered from very critical symptoms, which occurred a few days after the operation.

Richard H., aged eight years, began to complain of sore-throat on Friday, the 13th of October. Dr. M'Kinlay of Barrhead, near which place he resided, was sent for, and discovered a diphtheritic effusion on the tonsil, and prescribed chlorate of potash. On Sunday the disease had extended considerably, and that evening Dr. Gairdner saw him in consultation with Dr. M'Kinlay. They both considered it a very severe case of diphtheria. Iodide of potassium was added to the former prescription, and it was agreed that if the larynx became affected surgical aid should be called. At one o'clock on the morning of the 17th a messenger was sent for me, and I drove out to Barrhead, where I met Dr. M'Kinlay, and along with his son we went to see the patient. The progress of the disease was but too evident. The little fellow was in urgent distress, tossing about for breath, and though the pulse was fair, much worn out with restlessness and want of sleep. The steady advance of the symptoms clearly showed that the end was not far off, unless he could be relieved. Dr. M'Kinlay had already explained matters to Mr. and Mrs. H., so little time was lost in preparation. For the first time I gave chloroform in this case, and I was so much pleased with its effect that I would not hesitate to use it in future, although I have hitherto had some doubts of its applicability to tracheotomy. The operation was rather tedious, owing to the great depth of the trachea and some bleeding from small vessels, but I adhered to my rule not to open the trachea till I exposed at least half an inch, which I could see clearly at the bottom of the wound. The chloroform was a great help in this careful dissection. When the opening was made a great quantity of tough substance was expelled, and when the canula was introduced the little patient lay breathing quite quietly, and in a few minutes was fast asleep. I have observed this in almost every case. The patients are so worn out with tossing about in the struggle for breath, that they fall asleep as soon as respiration is rendered tranquil by the opening of the air tube. It was a great reward to see the satisfaction with which the parents regarded even this temporary measure of relief.

Next day I visited the patient along with Dr. M'Kinlay, who had remained almost constantly in the house since the operation. Matters were going on favourably, and breathing was tranquil. I took with me Mr. M'Farlane, one of my hospital dressers, who took immediate charge of the patient, and I was assisted by Messrs. M'Kinlay, Colquhoun, and Moffat, other students. Their assistance was of the utmost service, as the tube required to be frequently cleansed of the tenacious effusion which repeatedly almost obstructed it. The following day the edges of the wound became erysipelatous, and some emphysema of the neck took place, air escaping into the cellular tissue from the sides of the tube. The swelling partially dislodged the canula, and it could not be replaced in an entirely satisfactory position, so that a good deal of annoyance occurred from this untoward circumstance.

On the fourth day milk and other fluids were observed to come out of the wound along the tube, and I could not determine whether this was owing to the insensibility of the glottis, or whether the displaced canula had made an opening into the œsophagus from the pressure of its point against the back of the trachea. There was much inflammation and swelling of the neck, and consequent increased difficulty of breathing; still I hesitated to remove the tube, as it was frequently obstructed with tough mucus. Iodide of potassium was again ordered, and wine was to be taken at short intervals.

Oct 23rd. This morning, during a fit of coughing, the tube was expelled, and the patient, though exhausted by the effort, was relieved of much of his distress. When I saw him at midday, he was breathing very quietly, had slept a good deal, could swallow easily, and had taken a fair amount of nourishment, the swelling of the neck had much diminished, and he was altogether more comfortable. He cold take minced meat, soup, grapes, and wine, and was very willing to do all in his power to help himself.

Oct. 27th. Up till this date the improvement was almost uninterrupted. He slept a good deal, could take nourishment, had himself regularly shifted, and enjoyed the company of his brothers, and could whisper his wants. The swelling in neck almost gone, and breathing took place both through nose and through wound, which still remained open and could easily be kept free of discharge by a sponge pressed against it. His parents, as well as Dr. M'Kinlay, were sanguine of his complete recovery, indeed all the appearances gave ground for their opinion.

On Saturday the 28th, unaccountably, his breathing began to be rapid and laboured, his pulse got weaker, and Dr. M'Kinlay suspected the occurrence of effusion in the smaller bronchial tubes. The opening was still patent and not obstructed by any tough matter, but respiration became more and more feeble. There were no paroxysms of dyspnœa or coughing, but his strength began to sink, he was unable to take wine or other nourishment and he gradually sank exhausted on Sunday the 29th, thirteen days after the operation.

Case 25.—On the 18th of October Dr. Miller, of Castle-street, asked me to see a child aged 18 months, which he was called to attend two days previously. There were few signs either of diphtheria or croup, but very rapidly symptoms of obstruction in the glottis set in, and increased in severity up till the time I was sent for. When I saw it, the poor little thing was in great distress, but the pulse was fair. Although I formed a very unfavourable opinion of the case, I undertook the operation on the bare chance of saving the child from impending death. It was a very troublesome proceeding, and the introduction of the tube into such a small trachea was accomplished with considerable difficulty, but as usual I left the child asleep and breathing quite quietly. Respiration was free during the night, but next morning the secretion became viscid, and the peculiar whistling sound, so indicative of danger, came on. Pellets of hardened mucus were from time to time extracted from the tube, but soon the removal of these failed to afford relief. Breathing became weak and impeded, the child was soon exhausted, and died the same afternoon.

Case 26.—William K., aged 5 years, had symptoms of croup, a week before he came under my care. He was attended by Dr. Mather of Bellgrove, and was also seen by Mr. Turner. Medical treatment was persevered with up till the 4th Nov., when, finding the disease still progressing, the above-named medical gentlemen determined to place him under my charge, and sent him to the Infirmary, where he could get more constant attention than in his own house. I saw him at 8 p.m., and finding well-marked signs of tracheal obstruction, I at once performed tracheotomy. The operation, was unusually easy, the trachea being rapidly exposed, and the tube introduced without difficulty. I gave chloroform before operating, and was greatly satisfied with the result, which confirms the experience obtained in Case 24. The little patient fell asleep immediately after the operation, and continued to sleep for several hours. During the night the tube did not need much attention, the inner one being taken out and cleaned every two hours.

No untoward symptom occurred during recovery. From the date of operation patient continued to breathe easily through the tube, which was very easily kept clean. Several of the students volunteered their services, and each night till the tube was removed two were in attendance, taking watch alternately. · On the ninth day, that is, the 13th of September, I removed the tube, and the child breathed both by the wound and mouth. He could now take plenty of nourishment and was in every way favourable. He was dismissed with the wound completely cicatrized on the 28th November, quite restored to health.

I have only to add that in every case the temperature of the room was kept equable, about 65°, and a free supply of fresh air admitted by occasionally opening the door and window. The atmosphere was kept humid by allowing steam from the spout of a kettle to escape into the room. In most instances it was led by a tin tube to the vicinity of the patient's face, so that the air respired was both warm and moist. I still think this a safe precaution, and I am sure I have seen good result from it. At all events, when there is much tendency for the secretion to become dry, the moisture contained in the inhaled air softens it and allows of the more easy cleansing of the tube.

I again take the opportunity, which this paper affords, of impressing on my professional brethren the importance of having recourse to tracheotomy in a comparatively early stage of the disease. Whatever may be the primary affection, whether sthenic croup, diphtheritic croup, or genuine diphtheria, death occurs in fatal cases from one of two causes, viz., exhaustion or suffocation. At a comparatively early period in each case, the medical attendant can observe to what type the disease tends; and what I would still urge is that in those cases in which the tendency is to laryngeal or tracheal obstruction, the medical man should advise the surgical operation, when he has found medicine unavailing and before the energy of the patient has been wasted by the progress of the disease. I am the more anxious to press this view on the profession, because I have no opportunity of seeing these diseases in practice at their outset, and consequently have no right to make any suggestions as to their medical treatment. As I restrict myself rigidly to the practice of surgery, it is only where the physician or general practitioner has deemed surgical interference called for, that I have seen much of these fatal affections. I therefore feel justified in calling the attention of those into whose hands is committed the treatment of croup and diphtheria in their early progress, to certain conclusions which have been forced on me while viewing them in what I may call the terminal or surgical stage.—*Glasgow Med. Journal, Jan.* 1866, *p.* 399.

37.—ON HEPATIC DISEASE AMONGST EUROPEANS IN THE EAST INDIES.

By Inspector-General Sir Ranald Martin, C.B., F.R.S., Physician to the Council of India.

[Sir Ranald Martin states that very early in his course of service in India he became aware of the inutility of mercury in the treatment of chronic enlargement of the liver, and of its baneful effects in the splenic cachexia with hypertrophy of that organ. He therefore employed the nitro-muriatic acid in the form of bath, as follows:]

Directions for Preparing and Using the Nitro-Muriatic Acid Bath.—Take of pure concentrated hydrochloric acid, by measure, three parts; strong nitric acid, two parts; mix the acids very slowly and carefully, so as to avoid any evolution of heat or steam; after half an hour add the distilled water, five parts. Mix the whole carefully.

The Sponging Foot Bath.—1. Of this—the dilute nitro-muriatic acid—three ounces by measure are to be added to each gallon of water, to form a bath.

2. Two gallons of water may suffice for an ordinary foot-bath.

3. The bath thus prepared may be made to keep in use for a few days by adding to it, once each day, half an ounce of the dilute acid and a pint of water, in order to make up for waste in evaporation.

4. A portion only of the bath—say one-fourth—is to be well heated for use, and added to the remainder, so as to make the whole up to 96° or 98°.

5. Glazed earthen or wooden vessels should be used for baths; and the sponges and towels kept in cold water, lest the acid corrode them.

Manner of Using.—6. Let both feet be placed in the bath, while the inside of the legs and thighs, the right side, over the liver, and the inside of both arms, are sponged alternately; or let the abdomen be swathed in flannel soaked in the bath fluid. The process should be continued for half an hour morning and evening.

7. While using the bath, a gentle saline aperient, as Cheltenham or Epsom salts in some bitter infusion, or else Pullna water, should be taken every other morning: and should there be dryness or harshness of skin, a vapour bath at 100° or more,

used twice a week, will be found of much service in stimulating and opening the pores, and in purifying the surface of the body.

The General Bath.—8. In urgent cases, a general bath to envelope the whole body should be used, the proportions of the dilute acid and water being continued as above stated, adding one ounce of the dilute acid and two pints of water every day to make up for waste in evaporation.

9. The acid mixture forming the bath should be heated in earthen vessels, such as large pipkins; and the temperature of the bath should be measured by the thermometer at from 96° to 98°, as the body will be chilled by a degree of warmth which feels comfortable to the hand.

10. When the general bath is used, the patient before going into it should be covered over with blankets until a gentle perspiration is induced.

11. When in the bath, a covering-blanket should be drawn over the head and shoulders to confine the steam, and enable the patient to inspire it. A nightcap should be worn to protect the head from damp.

12. Before quitting the bath, the bedclothes, under-vests and drawers, and the towels should be ready warmed; the body to be dried while standing in the bath, and the dressing to be performed immediately, in a room well warmed.

13. Where the acid bath excites much irritation of the skin the quantity of the dilute acid may be diminished; and where irritation of the gums with general malaise occurs, the use of the bath may be relinquished for a time, resuming its application, if necessary, when the above symptoms have subsided.

14. The bath should be made of well-seasoned wood, the pieces dovetailed, and, if nails or screws be used, they should be well covered, and the crevices be made water-tight by putty, a layer of paint being placed on the outside.

15. The bath should be no larger than is absolutely needed to contain the person, with so much fluid as will cover it up to the neck. The height and breadth of the person should be carefully measured.

The following dimensions were used by a person 5 ft. 4 in. in height:—Inside length at the top, 5 ft. 4 in.; inside length at the bottom, 4 ft. 1 in.; inside breadth at the head, 1 ft. 4½ in.; inside breadth at the foot, 1 ft.

The physiological actions and uses of the aqua regia, in the form of bath, were, towards the end of the last century, brought to the notice of the profession, in the treatment of chronic diseases of the liver, by the late Dr. Helenus Scott, of Bombay, who believed that there was "a correspondence in the effects of the two remedies"—namely, the acids and mercury. Early in the present century, Dr. Scott made trial of his remedy on the person of General Wellesley, then arrived at Bombay, so far on his way to England, the medical officers having declared such a measure necessary to the restoration of his health. His disease was enlargement of the liver; and as he stated to his friends that he had then no desire to go home, they recommended their physician and his new means of cure to his attention. General Wellesley followed the advice of his friends, and in less than two months he returned to his command in complete health, and afterwards fought the battle of Assaye. This event was, in fact, the turning point of his fate.

Dr. Scott, who had for years employed this remedy in his own person and in those of friends suffering from hepatic enlargement, at length published the results of his experience. He stated that in a constitution broken down by disease, by the use of powerful remedies, such as mercury, or by the long continued action of the poison of syphilis, the acid treatment was quite as beneficial in the climate of England as in that of India, and the observation has been confirmed in our own time by other authorities.

"I know of no other means," he says, "that are capable of producing effects at once so salutary and so considerable, so free from injury, with so little inconvenience or disturbance. ......As with mercury, the system should be charged with it for a longer or shorter time, according to circumstances. In short, and as a general rule, I have found the acid bath advantageous and salutary in all cases where mercury is useful, and with the additional advantage that the acid treatment is attended by neither injury nor disadvantage. *By the harmless remedies that I now recommend, much good may be done in some diseases that are acknowledged to be beyond the ordinary means of relief.*"

"Even in irremediable cases of chronic enlargement of the liver," says Dr. Christison, "it proves useful in cleaning the tongue, improving the appetite, abating thirst, and sometimes in retarding the progress of disease."

"The rule," says Dr. Bence Jones, "is, sulphuric acid to astringe, hydrochloric acid to promote digestion, nitric acid to promote secretion."

The external application of the acids in the form of bath, is I repeat, by far the most effective mode of using this most valuable remedy; but I have observed that where the internal exhibition is preferred, a far longer time is necessary to the cure. For the dispersion of the products of inflammatory effusion into the viscera, whether the subject be anæmic or otherwise, the use of the bath proves of most excellent effect, and its resolvent power is often accelerated and increased by alternating its use with the alkaline renal depurants, or with taraxacum and bicarbonate of soda, as eliminants, and as means to saponify the ducts. Dr. Jephson, of Leamington, has assured me that in diseases of the mesenteric glands, and in swellings of the cervical glands also, he has found the swathing of the abdomen and the neck with muslin soaked in the warm solution of the acids, as used by me for the bath, of surprising efficacy, only covering the damped muslin with oiled cloth.

Dr. C. J. B. Williams considers nitric acid to be the best medicine he knows for the state of convalescence from inflammation, and in the various cachectic states following on acute disease or on habits of intemperance; and offers the suggestion, that its beneficial influences may be referred to its possessing a large proportion of oxygen in loose combination, and that it thus promotes a more free circulation through diseased parts, or through effused solids, by further oxygenating them. This distinguished physician, speaking of the nitro-hydrochloric acid and chlorate of potash, says: "It seems most probable that these agents are chiefly useful in supplying to the blood the oxygen necessary for the formation of fibrin or deutoxyde of protein, the respiration in its weakened state being unable to furnish a due amount."

But, however the physiological actions of the acids may be interpreted, the fact has long been established in my experience, that chronic diseases of the liver, with or without general cachexia, are absolutely curable by the remedies in question, while they have resisted all and every other means in ordinary use.

The powerful curative effects of the nitric and muriatic acids, in certain forms of stomach disorders, have often been commented on by medical writers and practitioners; and it is probable that many of the cases thus noted have been complicated with hepatic disorders. The general fact, Dr. Budd says, has been fully established by Dr. Prout. "Prout found them of especial efficacy in the gastric disorder that occurs in what is termed oxalic diathesis; and that is marked chiefly by distressing flatulence, and palpitation or irregular action of the heart, occurring some time after meals, and by the presence of oxalate of lime in the urine. The mineral acids are often useful to persons in whom digestion is habitually slow and feeble from a scanty secretion of gastric juice, and who have a sense of weight or oppression of the stomach after meals. They are often useful, as Pemberton showed, in the indigestion, attended with excessive formation of lactic acid, that occurs in weak and nervous persons, and where the stomach has been for some time disordered and weakened by a source of irritation elsewhere."

M. Trousseau has for years exhibited the hydrochloric acid after meals, he says, with much advantage. In an anæmic patient afflicted with obstinate chronic diarrhœa, he gave chalk at the commencement of the meals, and hydrochloric acid after them, with the result of a complete cure. He adds: "I do not wish to go beyond the fact, and only repeat that in the different forms of dyspepsia connected with chronic affections, whether of the thorax or abdomen, hydrochloric acid, taken after meals, may lead to therapeutic results deserving attention."

When the bowels are inactive, and the elimination by the acids unaccomplished, I recommend a mild aperient every other day, aided sometimes by colchicum. In moderate doses, the preparations of colchicum, added to taraxacum, when used with the acids or the alkalies, form a valuable combination in either case. In persons suffering from hepatic disease, and who are of a gouty or rheumatic habit, the advantages of the addition need not be insisted on.

Referring to the use of local and counter-irritant means, it is here deserving of mention that the first Dr. Jenner employed strong tartar-emetic ointment to the right hypochondrium. This he used perseveringly in chronic enlargement of the liver, and with marked benefit, as he declared.

The disuse of so powerful a means as the nitro-muriatic acid must be mainly ascribed to the brief, imperfect, and desultory manner in which it has generally been employed, whether internally or externally prescribed. It is only by those who have attentively observed the effects of this remedy during months together, in many cases, that its salutary influences in bringing about the removal of disease and in maturing convalescence can be justly appreciated. Certainly, had mercury been used in the irregular and careless manner spoken of, without regard to rational persistence or curative result, it never could have attained to any repute in the practice of medicine, in the treatment of acute or chronic disease.

The nitro-muriatic acid will occasionally, like mercury, irritate the mucous surfaces; and, like mercury, it acts on all the secreting organs; irritation of the gums and fauces, of a very harmless nature, occurring only after a protracted use. It is unlike mercury in that, after an observation of its actions and uses during more than thirty years, I am unable to recollect any one instance of its injuriousness.

When the patient is in so weakened a condition as to be unable to bear the immersion or the sponging, I have recourse to a swathe damped in the solution, worn round the body, and covered with oiled silk. This may be continued for any length of time, and great advantage is often the result.—*Lancet*, *Dec.* 9, 1865, *p.* 641.

38.—ON WHEAT PHOSPHATES.

By Dr. Tilbury Fox, London.

[Several years ago Dr. Hake suggested the production of a preparation containing the organised chemical products residing in the outer layers of the wheat grain, believing they would be found to possess peculiar nutritive qualities. During the last few years Dr. Fox has used these *wheat phosphates*, as the preparation is named, with great success.]

Whilst the subject of infants' food has, on account of prolific inventions and suggestions, become rather a bore, the tendency of refinement has been in the wrong direction, and very nice looking products, pleasing to the eye, but useless for nutrition, have been obtained. The various forms of infants' food are in the great majority of instances simply and purely starch, the central portions of the cereals consisting entirely of the latter products; these foods do not deserve the name of, and *are not*, *flours*; to call them flours is nothing more or less than *fraud*. Now, inasmuch as the starchy element is not the assimilative nor the flesh forming, but the heat and fat producing principle, all our past efforts to secure a nice white flour have been antagonistic to the possession of nutritive material, and actually the very desirable part of the grain contained in the bran—viz., the organised phosphates and other principles, have been deliberately rejected. Seconds flour makes a much more wholesome bread than that of the first quality.

The importance of a due supply of phosphates in health and disease has been fully recognised at the present day, and as a result we now possess various pyro-, super-, and hypo-phosphates and phites.

My experience teaches me that there is something essentially special in the organised phosphates—those in fact *which have been formed by passing through a living organism* (in Nature's own laboratory)—as compared with artificially-prepared phosphates. It is not the amount, but the kind exhibited that produces the good result. No simple mixture is in any way a substitute. It is not at all unlikely that the cerealin, of which little is known, but which is associated with the phosphates in the bran, and has an action similar to pepsin, may conduce to the beneficial result. A similar kind of action is observed in those cases where quinine fails, but decoction of bark instantly succeeds. The organised phosphates I speak of aid the assimilative function, and I find that ordinary foods are digested, and even medicines—for example, iron when given in combination—act most efficiently after they have previously failed alone. The same is the case with ordinary food in weak digestions. Many of the "foods" recommended are really animal in character—such are the extracts of meat. Nature distinctly points to the agent now under notice as a preferable aid, in virtue of its vegetable origin. The wheat phosphates, too, contain the desirable properties of brown bread without the objectionable ones—viz., the cuticle and husk.

The mode of preparation is simply to make a decoction of well-selected bran, carefully evaporate in a water bath, mix the residue with sugar, and reduce to powder. It may be used in the place of sugar, a teaspoonful or less being added two or three times a-day to the child's food. The cases in which its use is chiefly indicated are those amongst the young, in whom the assimilative function is at fault. I can speak very strongly in cases which belong to my own particular speciality—diseases of the skin. Eruptive diseases of the scalp in infants are are most frequently associated with faulty assimilation. Here the wheat phosphates act marvellously well. But in rickets, in marasmus, chronic diarrhœa, and impaired nutrition of all kinds, I believe them to be most valuable adjuncts. Pallid children pick up tone, colour, and flesh; worms disappear; intestinal irritation subsides; the secretions become healthy; and disease goes. I lay great stress on the phosphates under notice in their character as *organized products* as greatly helping assimilation of food and medicinal agents, and believe them to be the most preferable form of phosphates, especially for the young.—*Med. Times and Gazette, March* 17, 1866, *p.* 278.

DISEASES OF THE URINARY ORGANS.

39.—ON THE SYMPTOMS ARISING FROM ALKALINE URINE.

By Dr. H. Bence Jones, F.R.S., &c.

The bladder in health contains a feebly acid saline fluid. If the acid or salts vary greatly on either side of the usual amount the bladder is irritated by the unaccustomed stimulus of pure water or excess of salt. Thus a very strong solution of salts in the urine, or very dilute almost watery urine, cause irritation. Highly acid urine and, above all, ammoniacal urine have the same effect. Increased irritation causes increased secretion, and the mucus cells when thrown out in quantity acted on by the carbonate of ammonia become ropy and adhesive: like the ordinary alkaline mucus from other mucous membranes, as from the nostrils. If the irritation is continued pus is formed, and these cells also when acted on by carbonate of ammonia become ropy, and this has also been included under the term of ropy mucus. According as more or less earthly phosphates are present—that is, according as more or less magnesia or lime is taken into the system, more or less phosphatic precipitate is mixed with the ropy mucus or ropy pus, and this chemical error mounts up to the formation of mechanical masses of mortar-like or stony substances, which cause excessive pain either in passing through the irritated urethra or in scraping the inflamed surface of the bladder, or even by precipitation within the mucous membrane of the bladder itself.

The constant muscular contraction to expel the irritant is more disturbing to sleep than the cough of bronchitis ; and hence loss of rest becomes sometimes the most serious consequence of the complaint. Next to this is the extension of the inflammation upwards to the pelvis of the kidney. The ureters may show or not show that they have been inflamed ; sometimes no trace can be found of the passage of the inflammation upwards, but pyelitis and tubular nephritis producing pus throughout the kidneys and uræmia throughout the body may put an end to the progress of the disease by destroying life.

The inflammation may spread downwards, and then the urethra suffers, and often violent orchitis is set up.

If the neck of the bladder and the prostate get thickened by interstitial deposit of fibrin, the bladder is unable to empty itself, and the fluid that remains is in a variable state of more or less complete putrefaction. If the fundus is chiefly thickened by deposit and increase of the muscular coat it will contain less and less fluid as the thickening increases ; from either cause the frequency of the calls increase, and when ulceration takes place the amount of pain with each contraction becomes excessive.—*Med. Times and Gazette, March* 3, 1866, *p.* 220.

40.—ON THE TREATMENT OF AMMONIACAL URINE.

By Dr. H. Bence Jones, F.RS., &c.

The treatment of ammoniacal urine resolves itself into keeping the urine acid and stopping the inflammatory action in the mucous membrane of the bladder ; and as the alkalescence by its chemical action keeps up the inflammation, the stoppage of the formation of carbonate of ammonia in the bladder becomes the main object to be attained.

Usually, as the urine comes into the bladder, it is acid ; as soon as it gets there it finds some ammoniacal urine there already ; this not only neutralises the acid that comes from the kidney, but sets up decomposition of the urea, and forms fresh carbonate of ammonia; so that the removal of the ammoniacal urine that remains in the bladder is absolutely necessary, unless sufficient acid can be made to come from the kidney to neutralise it.

Vegetable acids far more quickly run off in the urine than mineral acids, of which sufficient cannot be taken to pass in quantity through the kidneys. Of all the acids lemon juice is perhaps the best which can be used. Many years since Mr. Witt made the following analysis of lemon juice for me (see *Quarterly Journal of Chemical Society*, vol. vii., p. 44);—One ounce (480 grs.) of lemon juice contains only 1·728 grs. of inorganic constituents; of which potash, sulphuric and carbonic acids constitute three-fourths; phosphoric acid, soda, and lime, with traces of silica, and iron and magnesia, constitute the other fourth. Hence water and citric acid constitute by far the largest part of the lemon juice, the citric acid being, on an average, about 40 grains to the ounce of juice.

It is highly probable that citric acid and other vegetable acids have an anti-inflammatory action (lessening the oxidation that is going on) wherever they are carried, and they not only pass to the kidneys, but probably diffuse into every part of the mucous membrane of the bladder; to a small degree lessening the alkalescence of the textures in which the peroxidation is going on.

The citric acid must not be taken in such quantity that it irritates the stomach and bowels, nor should it set free uric acid in the urine. So that the highest limit can be easily recognised by the bowel symptoms. The lowest limit is so much as will just make the urine acid. This cannot always be reached. Three, four, five, ten lemons or more daily will not neutralise the alkali in the urine, and will not so stop oxidation and nutrition that the body will become feeble and wasted. Before this effect is produced some other way of removing the ammonia must be tried. Taking away the alkaline urine by the catheter so frequently that a smaller quantity of lemon-juice will keep the water acid is easy when the passage of the catheter does not cause mechanical irritation to take the place of the chemical irritant. The catheter must be used more or less frequently, according to the reaction of the urine. If drawing off the water at night whilst taking lemon-juice or other acid keeps the urine feebly acid, this is all the mechanical means required; if not, then, twice thrice, four times daily the water must be drawn off; and even, in extreme cases, the catheter must be kept in the bladder, so as to free the urine continually without the irritation of the passage of the instrument; but the presence of the foreign body in the bladder, except when perfect rest is observed, often causes so much irritation that it is an aggravation of the disease. Moreover, by washing the bladder with small quantities of hydrochloric or acetic acid and water, after the urine is drawn off with the catheter, the ammonia may be easily neutralised, and a very small quantity of feebly acid fluid may be left in the bladder to counteract the decomposition of the urea, and to make the surface of the bladder itself less foul. How frequently the washing should be repeated, and what the strength of the acid injected should be, ought to be determined by the reaction of test-paper or the smell of the urine. Whatever you do, remember that the object is to get rid of a chemical irritant, and that if by rough handling of the catheter, by its too frequent use, or by too strong an injection you mechanically or chemically irritate the bladder more than it is irritated chemically by the ammoniacal urine, no good will come from your treatment.

When you have removed or can prevent the ammonia of the urine, then balsamic remedies, as in bronchitis, are often useful. These pass from the kidneys and also directly from the blood to the mucous membrane. Buchu, cubebs, copaibæ, turpentine, eau de goudron (purified tar water), all these may be useful; remembering here, also, that if the stimulant be too great, harm may be done by setting up fresh action. Astringents, as any preparation of gallic acid, tannic acid, uva ursi, rhatany, catechu, matico, alum, lessen the secretion of pus and mucus by contracting the textures.

Tonics also often are useful when the acute symptoms subside—as quinine, biberine, pereira brava, and other bitters.—*Med. Times and Gazette, March* 17, 1866, *p.* 275.

41.—ON THE SYMPTOMS AND CONSEQUENCES OF INTERSTITIAL NEPHRITIS.

By Dr. H. Bence Jones, F.R.S., &c.

If the disease could be limited to the interstitial texture, and to that alone, it is possible that no appearance of albumen in the urine might occur. No fibrinous casts might be seen. The kidney might gradually, by its contraction, cease to secrete urinous matter, and uræmia would be the final result. But the affection spreads to the vessels and tubes, and then the pressure in the tufts is altered and albumen is effused, the production of epithelium is increased, and some fibrinous casts appear in the urine.

Usually the specific weight of the urine becomes less and less, and generally this weight may be taken to be directly proportional to the amount of cortical structure that exists; so that when the specific weight of the urine is least the cortical structure is least also. Even when hardly any cortical structure is left, an excess of water of the lowest specific gravity may be thrown out, most probably from increased circulation in the mammary part of the kidney.

As long as this excessive flow continues no dropsy occurs, and hence throughout the whole course of the disease no dropsy may occur. The altered blood and the altered lymph may give rise to all kinds of hemorrhages, thickenings, and effusions, and chemical actions on the nerves and muscles, which may be summed up in the word poisoning, and which I have already brought before you when speaking of uræmia.

At any period during the progress of the disease more acute inflammation may suddenly come on, and in the cellular tissue, more readily even than in the tubes, pus-cells are readily formed, suppuration extends throughout the altered cortical structure, and rapidly that assemblage of low feverish general symptoms which now appears to us to be so inaccurately designated as a typhoid state, is produced.

With regard to treatment, medicine avails little for stopping the effect for alcoholic poison or for removing the thickening of the interstitial structure. No appreciable alteration of structure occurs until the alcohol has acted for months ; but as each day produces its infinitesimal effect some good may be done by recognising the disease early and by stopping as far as possible the further action of the poison. Usually the second nature cannot be changed, and the treatment consists then only in alleviating symptoms and in warding off complications as they arise.

Of all the symptoms that of general debility is the one which will most frequently present itself to you, and of all the complications uræmia is the one which is most serious.

The loss of general mechanical power consequent on wrong chemistry of the blood and lymph begins from the very commencement of the disease, and lasts until its final close.

One of the first effects in the blood is the insufficient production of blood globules, on which the passage of oxygen into the blood depends. The gradual diminution of albumen in the blood is a very much more remote cause of symptoms, and as long as food can be taken this admits of an easy remedy ; whilst the reproduction of blood globules requires a far more complex chemistry, and necessitates an increased supply of iron for the formation of hematosine.

The accumulation of descending substances in the blood, and still more in the lymph which surrounds and permeates each particle of every structure, must begin from the moment that the kidney begins to perform the function of oxidation and excretion imperfectly. Still it is evident from the experiments in which one kidney is removed and poisoning occurs that even half the structure may be destroyed by disease without the occurrence of bad symptoms. This is a large margin for safety, but as soon as it is removed the lymph begins to cause wrong chemistry in the textures. Products of downward change from urea upwards keep circulating in the blood and lymph, and these substances produce their chemical actions everywhere. Thus on each mucous and other membrane urea exudes, and in the mouth even it is converted into carbonate of ammonia, and this, with some urinous smelling substance, gives the foul breath that is so frequently observed. The same substances effused in the stomach give rise to sickness, and in the bowels often to diarrhœa. The higher uric acid compounds in the joints produce chronic gout, and the excess of kreatin in the muscles may stop the changes in the syntonin, whilst the still higher compounds as yet uninsulated may in the nerves and brain produce irritant and narcotic symptoms from the slightest cramp or subsultus to the most profound coma. That there is in every case some distinct local action is clear from the amendment or arrest of the symptoms which is occasionally observed. The sickness and the diarrhœa may be stopped, the convulsions may cease, the coma may disappear.

Let us look at the removal of the poisonous substances a little closer. The vomiting and diarrhœa are not to be checked by strong remedies when the ammoniacal and urinous odour tells you of the accumulation of poisons within. You must, whilst endeavouriing to obviate the extreme prostration which these actions cause, remember that more serious muscular and nervous actions are at hand, and that except by acting on the skin, you have no other gate through which the poisonous substances can pass out. Warm baths and hot air baths give no immediate relief like vomiting and purging. I have been asked by a patient for an emetic when he was within a few moments of his death from exhaustion. Almost whilst speaking to me another violently relaxed action of the bowels came on, and from the effects of this he died.

By the stomach, by the bowels, and by the skin, all poisonous substances can be thrown out of the blood and lymph, and, according to circumstances, by this or that road a passage must be obtained.

Irritative actions of the nervous system seem to mark a less degree of affection of the nerves than narcotic actions, and cramps, subsultus, and epileptiform convulsions pass away or yield to treatment far oftener than oppression and coma.

To any who have witnessed the stoppage of convulsions in Bright's disease by the inhalation of chloroform, the thought must have occurred that the chloroform must have so acted directly on the nerve as to render it incapable of being irritated by the urinous poison. A volatile coma, so to say, is produced. Perhaps the local action of the urinous poison is checked by the substance on which it was acting being altered in composition by the chloroform. But when the urinous poisons are causing both convulsions and coma, then this fixed coma is more dangerous than the convulsions; and all your efforts must be directed to stop the local action on the nerves, and whilst doing your utmost to evacuate the poison, you must try, by counter-irritation, to determine by blisters the largest possible effusion of serum and lymph as near as possible to the spot where the wrong action is taking place.—*Med. Times and Gazette, Jan.* 6, 1866, *p.* 1.

42.—ON THE TREATMENT OF TUBULAR NEPHRITIS.

By Dr. H. Bence Jones, F.R.S., &c.

[Tubular nephritis is to the kidney what bronchitis is to the lung. In its comparative mildness, in its occasional intensity, in its long duration, and in its rapid and perfect cure it resembles slight, severe, chronic, and rapidly curable cases of that disease. Cold is the most frequent exciting cause, then scarlet fever. In the treatment of the disease,]

The first indication is, as far as possible, to remove the causes of the attack. When cold is the cause, warm baths, vapour baths, and warm clothing may help to restore the action of the skin. Tartarised antimony has no strong action on the kidneys, and has a very decided action on the skin; and when it acts on the bowels or on the stomach, causing vomiting, it removes urinous substances from the blood. Hence it is a most important remedy in the acute dropsy from cold. Even when poisonous substances are passing out of the blood, as after scarlet fever, cantharides, turpentine, and in extreme cases tartarised antimony may sometimes be used. In slight cases simple dilution with the purest water washes the impurity out of the blood.

The second indication is to stop the thickening of the tube, and to relieve the obstructed vessels. Abstraction of the blood by cupping glasses on the loins can hardly be expected to effect the circulation in the tubes of the cortical structure of the kidney; whilst general bleeding, even to a few ounces, has a distinct effect on the pressure of the blood in the malpighian tufts. Hence, if blood at all is taken, it should be by venesection. Digitaline in small doses acts on the nerves that regulate the circulation rather like a stimulant, but in large doses or long continued the pressure on the arteries is diminished; and of all diuretics this is the only one which is admissible in acute tubular nephritis. As the disease becomes more chronic iodide of potassium may be used in diuretic doses. In ten minutes after the first dose it is present in every part of the kidney, and probably may be found there for many days after the last dose has been taken.

The last indication is to relieve the symptoms and complications that occur.

Of these, the dropsy is the most important. In the acute stage you have to contend with the urinous dropsy. Strong vapour baths or hot-air baths, used to the greatest extent that the strength will bear, are most efficacious. Strong action on the bowels by those watery cathartics that have the least action on the kidneys—as, for example, jalap, gamboge, and elaterium. If the strength admits of it, emetics, tartarised antimony, and ipecacuan may be given.

When the acute stage is over, the tendency to anæmic dropsy begins. To prevent this, and after the anæmic dropsy is set up, small quantities of iron should be given. Nitre, cream of tartar, broom tea, and other diuretics may be used, sometimes in very large doses. Even tincture of cantharides may, in the more chronic state, be prescribed. In a very short time the whole of the anæmic dropsy may be removed, and then iron in larger quantity should be given to prevent a return of the effusion.

The treatment of the secondary inflammations should be carried on with allowance for the general debility which will appear when the increased arterial action subsides.

Counter-irritation is to be preferred to local bleeding, because it saves the red blood. Mercury is hardly ever to be used, as in this form of disease its poisonous action is most quickly and violently set up.—*Med. Times and Gazette, Jan.* 13, 1866, *p.* 32.

43.—ON SECONDARY ALBUMINURIA (CAUSE NOT PRIMARILY IN THE KIDNEY).

By Dr. George Harley, F.R.S., Professor in University College, and of University College Hospital.

Albuminuria from Imperfect Digestion.—Every one who has had much experience with diseases of the urinary system is aware that cases of temporary albuminuria are occasionally met with in persons whose only symptoms are referable to disordered digestion. It may be that they are usually dyspeptic, or, what is still more frequently the case, they have been indulging in food that has disagreed with them. Thus, cheese has been known to cause albuminuria in children, and lobster or crab in grown-up people. In these cases there is nothing essentially wrong with the kidneys, and the albuminuria is simply due to the endosmotic equivalent of the albumen absorbed into the circulation being different from what it ought to be. Certain kinds of albumen, as before said, will not stay in the blood unless they have previously undergone modification by the digestive process. Hence, if the necessary modification be interrupted, either on account of the digestive organs being out of order, or by reason of the indigestible nature of the food itself, a temporary attack of albuminuria is the immediate consequence.

The effect of food on albuminurias of all kinds is much more important than most persons imagine; for, as I shall afterwards show when on the subject of treatment, by diet alone we can at pleasure augment or diminish the amount of albumen eliminated with the urine. Nothing further need therefore at present be said on the subject.

Albuminuria from Nerve Lesion or Reflex Irritation.—This, like the last form of the affection, is characterised by the absence of organic change in the structure of the kidneys. But the analogy does not proceed further, for although the albuminuria is due to no structural change, it can, nevertheless, be clearly traced to a disturbance of the renal circulation. It may be remembered that while on the physiology of the subject I mentioned that in the lower animals albuminuria can be produced at will by a variety of operative procedures—such, for example, as section of the renal nerves, division of the spinal cord, lesion of the cerebral peduncles, &c. So we cannot be in the least surprised at the detection of albumen in the urine of patients labouring under similar nerve lesions, the result of disease. As the cause of the albuminuria is apparent enough in the lower animals—namely, renal congestion—there need be, I think, but little doubt that in man the pathology of the morbid condition is precisely similar. Cases of albuminuria in connexion with spinal paraplegia, hemiplegia, apoplexy of the base of the brain, &c., have been so frequently reported, that nothing further need be said upon the subject, but we may at once pass on to the consideration of that form which is least understood—viz., the albuminuria of pregnancy.

Albuminuria of Pregnancy.—Many years ago, when House-Surgeon to the Edinburgh Royal Maternity Hospital, I was led, at the suggestion of Professor Simpson, to examine the urine of every patient that entered the charity during three months. The urine of those who came in after the pains of labour had already begun was immediately drawn off by the catheter, so that even in their cases all risk of error, from the accidental admixture of vaginal secretions, was avoided, and I was surprised to find how much less frequent the presence of albumen in the urine of pregnancy is than I had been led to expect from the opinions expressed by previous observers. It did not amount to 4 per cent., which is actually less than a third the percentage of the cases of temporary albuminuria found to exist among female patients admitted into a general hospital. As regards the frequency of puerperal convulsions, an almost similar conclusion was arrived at—namely, that they are not so exceedingly common as generally supposed, for although I have seen them both in private and public practice, not one case of the kind occurred among the 450 women who were at that time delivered under my superintendence, and at least ten of whom were passing albumen in the urine. Moreover, even in those cases where the albuminuria is most severe, accompanied by dropsy, and apparently entirely due to the pregnancy, there may still be no convulsions, as the following case will show:—On May 30, 1851, a servant-girl, aged 20, was sent into the Edinburgh Infirmary by a medical man who thought her case was one of Bright's disease. Her urine was highly albuminous, her face puffy, her legs swollen, and her abdomen greatly distended. On passing my hand over the abdomen I imagined that I felt an enlarged uterus, but on suggesting pregnancy, the girl indignantly denied its possibility. It was true that she had seen nothing for the last nine or ten months, but to that she attached no importance, as she had often before been irregular. She had never had morning sickness, never felt any movement, and besides, it was impossible she could be pregnant. Facts, however, are stubborn things, and fœtal hearts being not usually heard in cases of simple ascites, I quietly, but firmly, told her my opinion that the albuminuria was associated with, if not the direct result of pregnancy; at the same time adding that she might remain in the hospital a few days while we tried to relieve the dropsy. In four days she was taken in labour, and within an hour after I was called to her bedside she was delivered of a full-grown healthy child, without a single convulsion or any head symptom whatever. From this time the albumen began to diminish in the urine, the dropsy to disappear, and on June 16, twelve days after delivery, not a trace of matter coagulable by heat and nitric acid was to be found in the urine. On July 14 she was dismissed in apparently perfect health.

[Dr. Harley then relates two other cases, and after detailing the results of various investigations, draws from them the following general conclusions.]

1st. That in the albuminuria of Bright's disease there is less than the normal amount of albumen in the blood.

2nd. As the amount of the albumen in the urine increases, the quantity in the circulation proportionately diminishes ; and as the albuminuria decreases, the amount in the circulation gradually reapproaches the normal standard.

3rdly. The quantity of albumen and urea in the urine are in inverse proportion to each other—that is to say, where there is much albumen there is little urea, and where there is much urea there is but little albumen.

Lastly. The condition of the urine affords us a pretty correct idea of the probable condition of that of the blood.

I have now to point out an error into which people are very liable to fall—namely, to look upon the albuminuria of pregnancy, and pregnancy with albuminuria, as one and the same thing, while, in reality, they are perfectly distinct. When we speak of the albuminuria of pregnancy, we mean that the pregnant state induced the albuminuria, as was the case in the example first cited ; whereas when we speak of pregnancy with albuminuria we simply mean that a woman during the period of her pregnancy has been attacked with kidney disease.

I have now to call attention to a third form of albuminuria and pregnancy—namely that in which a patient already the subject of kidney disease becomes pregnant. This is not only a graver complication than the "albuminuria of pregnancy," but, as a rule, even more dangerous, both to the life of mother and child, than a case of pregnancy complicated with kidney disease. The following case, of which I can only give a very brief abstract, will illustrate this remark :—

The subject was a lady, aged 30, whom I saw in consultation with Dr. Magrath, of Teignmouth, and to whom I am indebted for the patient's history. Her mother died of diseased kidneys at the age of 45. The patient herself was healthy until she suffered from frequent attacks of intermittent fever whilst abroad. During her first pregnancy her legs were noticed to be œdematous, but, as albuminuria was not suspected, the urine remained unexamined. Her delivery was followed by a convulsion, for which she was bled to sixteen ounces. During the second pregnancy and confinement, nothing remarkable was observed, but after the third delivery she was again seized with convulsions, which were on this occasion arrested with chloroform. In the early part of 1863 she again conceived, and almost immediately before or afterwards, was, while recovering from a catarrhal fever, seized with hemiplegia. In the following month she was seen by Dr. Brown-Séquard, and, under the free administration of tonics, with change of air, she gradually regained the power of her arm and leg. In August of the same year, shortly before I saw her, her face and hands were for the first time observed to be œdematous. The urine was of a specific gravity of 1020, and was not noticed to contain albumen. About this time she suffered from headache, vomiting, with stertorous breathing, and muscular twitchings during sleep. At the end of August, when I examined the patient, the specific gravity of the urine was 1014, contained a large quantity of albumen, coagulating to one-third of its volume, and with the microscope both blood corpuscles and renal tube-casts were detected. As the symptoms of uræmic poisoning continued to increase until the breath had a urinous odour, the induction of premature labour was had recourse to, during which she had a convulsion, which was arrested with chloroform. After this the patient made a slow recovery until October 24, when she was considered convalescent. The urine at this time was abundant, of a specific gravity of 1014, contained very little albumen, and and only a few hyaline tube-casts.

On November 25 the quantity of urine was forty ounces, and the specific gravity 1021. Waxy tube-casts were still detectable.

In this case, as in the preceding, the union of pregnancy and kidney disease must be looked upon as accidental. In the former the albuminuria supervened during the course of the pregnancy; in the latter, the pregnancy in the course the kidney disease, each being quite independent of the other. The distinction to be drawn between these cases and that first cited —namely, the albuminuria of pregnancy—is of the utmost importance both as regards prognosis and treatment; for while in the albuminuria of pregnancy it is to the pregnancy, and not to the kidneys, that we must look for relief, in the case of the kidney disease associated with pregnancy, as well as in that of the pregnancy associated with kidney disease, it is to the condition of the kidneys that we must specially direct our attention. The reason of this is easily understood when we remember that in the albuminuria of pregnancy the albumen rapidly disappears from the urine after delivery, whereas in the others the mere circumstance of delivery influences the albuminuria only in a secondary degree. The patient must be as carefully treated for diseased kidneys after the expulsion of the child as during its sojourn in the uterus.

As regards puerperal convulsions in cases of albuminuria, as far as my own experience goes—and I have nothing else to guide me—they are far more frequently the result of diseased kidneys than of the simple albuminuria of pregnancy. Convulsions, as we well know, frequently occur without pregnancy, and we cannot be surprised at their occurrence when pregnancy is superadded to the albuminuria. In the latter case the convulsive attack may be delayed until the time of delivery, or it may occur at any period of the pregnancy. There can be no doubt that albuminuria, from whatever cause, predisposes to epileptic convulsions, just as any other disease which impoverishes the blood, and thereby leads to malnutrition of the nervous system ; for malnutrition of the nervous system is of all things the most fruitful source of epilepsy. In the second case cited we had ample proof of the impoverishing influence exerted upon the blood by albuminuria ; and the absence of convulsions in it might probably be attributed to the circumstance that the blood had almost entirely regained its normal condition at the time of delivery.

That epileptic convulsions may occur in uncomplicated albuminuria in the predisposed I see no reason to doubt ; and a well-marked case of this kind, and which is briefly as follows, fell under my notice in 1851 :—

A dark-complexioned woman, of moderate development, who had been for nine years a strumpet—she was then 28—was admitted in April 28 into one of the wards under the charge of Dr. Halliday Douglas. She had a dull, languid, and waxy appearance. Her urine was albuminous, of a specific gravity of 1008, and for several days after admission averaged 100 ounces per diem. She said her illness began with dyspeptic symptoms eight months before, and that shortly afterwards she had a fit, which had since recurred about once in every three or four weeks. On May 11—that is to say, thirteen days after her admission—she had a fit which lasted an hour, and from then until she died—seven days later—she had one, two, or more every day. She died immediately after coming out of one. From the 11th to the 18th she was in a state of stupor, and was not easily roused out of it ; but when awake she answered questions correctly. The urine fell from 100 to 50 ounces a-day. during the last fortnight of her life. On post-mortem examination, the kidneys were found enlarged ; one weighed six, the other five ounces ; they were in a state of waxy degeneration. The woman had had syphilis. The capsule came off with difficulty ; the texture of the organ was exceedingly firm, and of a pale colour. The separation between cortical and medullary substance was ill-defined. In the pyramids were several opaque points which yielded a puriform fluid on pressure. When examined with the microscope, similar opaque points in the cortical substance were found to contain renal epithelium in all stages of degeneration, and numerous oleo-albuminoid granules.

Supposing this woman had chanced to become pregnant a month before she took ill, she would have been delivered just about the time she died, and her death would in that case have been most probably attributed to puerperal convulsions from albuminuria, whereas, as we here see, the pregnancy might in reality have had nothing at all to do with it. I do not wish it for a moment to be supposed that I ignore the influence of pregnancy, and more especially of the effects of delivery, in inducing convulsions in those otherwise predisposed to them.

All I desire is, to utter a strong protest against the oft-repeated statement that puerperal convulsions are always the result of the albuminuria of pregnancy, while in reality they are much more frequently the concomitants of true kidney disease, assisted by the effects of the puerperal condition, the chief cause of the convulsions being the retention in the circulation of the excrementitious urinary products.

It is of vast importance to both mother and child to be able to diagnose these cases; for while in the albuminuria of pregnancy, unless the symptoms be severe, the case may almost be left to nature, in pregnancy, associated with true kidney disease, energetic treatment is demanded, even, as we have seen, to the induction of premature labour; for even when the pregnancy does not end in convulsions it never fails to act prejudicially on the renal affection.

I would even go further, and say that pregnancy, in many cases, is one of the exciting causes, not alone of albuminuria, but of true kidney disease; just in the same way as cardiac and hepatic affections are—by keeping up renal congestion.—*Med. Times and Gazette, Dec.* 16, 1865, *p.* 651.

44.—ON THE TREATMENT OF BRIGHT'S DISEASE.

By Dr. George Harley, F.R.S., Professor in University College, and of University College Hospital.

[It is in the power of the enlightened practitioner not only to check the milder forms of renal disease, but even to control and mitigate the symptoms arising from the severer renal affections. To be successful in treatment, however, we must be correct in diagnosis. The first principle upon which we must act is to give rest to the diseased organs, reducing their labour to a minimum by promoting the vicarious action of other organs.]

There are three channels by which we can draw off from the system the excrementitious materials which normally fall to the lot of the kidneys to eliminate—namely, the bowels, skin, and lungs. By keeping up an excessive action in these, we can not only remove for a time a portion of the burden from the overtaxed kidneys, but in those cases where dropsy has already become a distressing symptom give great relief to the patient.

The value, indeed, of pressing into our service the vicarious action of the bowels, skin, and lungs in the treatment of renal disease it is scarcely possible to over-rate, for we can not only by such means give rest to the kidneys and diminish dropsy, but even mitigate the more distressing symptoms which are ordinarily included under the term uræmic intoxication, but which, more properly speaking, are the direct result of the combined effects of the retention in the circulation of all the excrementitious products, organic as well as inorganic, which normally fall to the lot of the kidneys to excrete. The skin and lungs are more powerful auxiliaries in the elimination of urinary products than is generally supposed, for, as shown in our first lecture, the cutaneous perspiration does not only carry off water, but many of the organic as well as inorganic urinary salts. Thus it has been found that the sweat even in health contains urea, uric acid, phosphates, and chlorides, while in disease, in addition to these, it contains many abnormal compounds, such even as the insoluble oxalate of lime. Pulmonary exhalation, too, as was then shown, may be almost of equal service, for in the expired air of even healthy man have been detected urea, uric acid, urate of soda, and urate of ammonia.

The vicarious action of the bowels is to be induced by the internal administeration of mild or drastic purgatives, according to the constitution and condition of the patient. When there is much dropsy, elaterium is a favourite form of purgative, but in cases of kidney disease it is usually advisable to administer it along with hyoscyamus, as it not unfrequently brings on an exhausting diarrhœa, especially if given after the symptoms of uræmic poisoning have already set in.

The vicarious action of the skin may be induced either by the internal administration of diaphoretics or the external use of the warm bath, vapour-bath, or hot air-bath. The two latter not only increase the cutaneous, but also augment the pulmonary elimination of urinary products. This is especially the case with the hot air-bath.

I may here remark that the usual practice of trying to diminish rather than to increase the urinous odour of the sweat and breath in cases of advanced kidney disease is greatly to be reprehended ; for instead of trying to check, we ought, on the contrary, to assist Nature in her laudable efforts to rid the circulation of the deleterious agents that are gradually extinguishing the life of the patient.

In the next place, the employment of antiphlogistics is in many of the inflammatory forms of kidney disease of essential service. The most powerful of these is, of course, the local abstraction of blood, either by leeches or the cupping-glasses. But just as in many cases of inflamed lung the general condition of the patient prevents the employment of such active means, so also in the case of the kidney we must occasionally abandon this line of treatment, and content ourselves by merely diverting for a time the course of the circulation, either by the application of dry cupping-glasses, counter-irritation, or of hot fomentations to the loins.

I ought not to omit to mention that the unloading of the portal circulation by a smart calomel purge will often prove an important adjunct to other antiphlogistic measures.

As regards the employment of diuretics in the treatment of kidney disease, a few words are here necessary. In the first place, it ought never to be forgotten that in acute Bright's disease, as well as in the first stage of all inflammatory and congestive attacks occurring in the course of chronic kidney affections, diuretics are inadmissible. In the second place, it must be borne in mind that great care should always be observed in their selection; for a diuretic which will prove beneficial in one form and at one particular stage of renal disease will often not only do no good, but actual harm, when administered in another form or at another stage of the same attack. Thus, whenever the albuminuria is the result of active congestion, the antiphlogistic variety of diuretic—such, for example, as a combination of bitartrate of potash and digitalis—is to be selected; whereas in the absence of active congestion, and more especially when the vital powers of the patient are low, the stimulating variety of diuretic may not only be used with impunity, but with actual advantage. The reason why the employment of diuretics often does harm in acute kidney affections, is readily understood when we recollect that they have always the tendency rather to increase than diminish the flow of blood to the already engorged organ.—*Medical Times and Gazette, Dec.* 30, 1865, *p.* 702.

SURGERY.

DISEASES OF THE BONES AND JOINTS, ETC.

45.—ON THE USE OF CHLORIDE OF ZINC IN SURGICAL OPERATIONS AND INJURIES.

By Campbell De Morgan, Esq., F.R.S., Surgeon to the Middlesex Hospital.

[The use of chloride of zinc was at first adopted only after removal of cancerous tumours by the knife, but the results obtained led to its application in all wounds, whether made in operations or accidentally.]

The cancer wards in the Middlesex Hospital are constantly forcing on the minds of the surgeons attached to it the questions, Can nothing be done to remove this fearful disease? Is, in the majority of cases, its recurrence after operation an inevitable necessity? May it not be that the apparent return of the disease is but the continued development of germs which were not included in the extirpation of the tumour? My colleague, Mr. Moore, has recently given expression to the conclusions at which he has arrived. They are so important as to demand the most careful consideration on the part of the profession, and, if correct, will reverse the decision on the question of removal, at which a large number seem to have arrived. I think that the observations and the reasonings upon them are such as to a great extent to justify his conclusions and to encourage the hope that we may yet, in cases admitting of operation, get a mastery over this hitherto unconquerable disease. But take what view they please, it is quite clear that surgeons should endeavour in their operations to remove *thoroughly* every trace of the disease. Probably the fact first shown by Schroeder van der Kolk has not been sufficiently considered, viz., that the germs of disease lie scattered far beyond the apparent limits of the tumour. But in addition to this mode of dissemination of cancer, it frequently occurs that in operations parts of the tumour are cut through, and are left to be removed when the principal mass has been taken away. In such a case it is probable that of the innumerable living cells which are set free and deposited in the wound some will find a nidus in which they will go on developing—they will be scattered like seed, in fact, and grow wherever they find a favourable soil. That this is very often the case has seemed to me shown by the rapid appearance of points of disseminated cancer in and around the cicatrix after operation. Were the return of the disease dependent solely on a constitutional tendency or peculiar condition of the blood, it is probable that the disease when it returned in loco would do so much as it appeared at first, in a single tumour, or that it would return in a similar part to that in which it first appeared, as in the opposite breast. But both are rare occurrences. I know not how far this view may be capable of proof, but every surgeon has seen cases which would be easily explained on such a supposition, and with difficulty on any other. It appears to me that it may be to this mode of dissemination and implantation of cancer-germs that we may attribute the wider and more rapid return of the disease in situations where there is much fat, and in the young, than in elderly and spare persons. In the young, of course, the general activity of natural growth is shared by any morbid structure, and we should expect to find a wider area of disease radiating from the principal msss, in the manner described by Van der Kolk. But something may also be due after operation to the laxity of tissue, and the freedom with which imbibition and infiltration may occur at this age. In the case of malignant tumours forming in the neighbourhood of fat, it is probable that the widespread and rapid recurrence of the disease may be owing to the laxity of tissue alone, and its consequent capability of ready imbibition.

A case occurred at the Middlesex Hospital which, at the time, impressed me strongly with the notion that cancer could be thus propagated by implantation. A stout, otherwise very healthy woman, aged fifty-six, was admitted in December, 1863. She had been operated on by my colleague, Mr. Lawson, in the previous September, for a large encephaloid tumour on the upper and anterior part of the thigh, which she had noticed for the first time only five weeks previously. The mass was connected to the fascia lata, above which it was embedded in fat; part of it had to be dissected away from the fascia after the principal mass had been removed. The wound healed rapidly, and she went out apparently well on the 13th October. On the 23rd November she noticed a return of the disease. On her admission the second time in December there were large nodules lying below the skin in the course of the cicatrix, and several smaller ones scattered about above and below. She had had no pain; had gained flesh since she went out; had a quiet pulse, a good appetite, and showed no appearance whatever of constitutional disturbance. The growth was removed the day after her admission, with a large quantity of the great mass of soft fat in which it was embedded. The outgrowth had again to be traced piecemeal, but it had now penetrated through the fascia and between the muscles. On examining the removed part I was struck by observing that extending widely through the fat were little deposits of the morbid structure in great number, quite distinct from one another, and varying in size from the smallest pin's head to a large grain of wheat; some of them were at least $3\frac{1}{2}$ inches distant from the central masses, and they may possibly have extended still further. But the point of interest was, that while the masses which occupied the situation of the original tumour were large and spreading, these little scattered deposits were small and of comparatively uniform size. The impression conveyed was, that while the larger portions had grown from palpable masses which had not been reached by the knife in the first operation, the smaller ones had originated in minute germs, which had been scattered baoadcast, as it were, in the soft structures around, and had permeated them to various distances through the delicate lax connective tissue. Of course, the disease rapidly returned, but still the parts took on a healthy action after the operation, and there was no indication of cancerous cachexia for some months.

I remember, too, a case which occurred very many years ago in the practice of Mr. Mayo, at the Middlesex Hospital. He removed a small tumour from the sole of the foot. At that time there was no appearance of disease elsewhere. In the course of three weeks little nodules of cancer appeared over the whole leg and thigh up to the groin; there were at least fifty of them. The glands in the groin enlarged at the same time; the man soon became cachectic, and died. Now, which of the two explanations is admissible? That which maintains that we had merely evidence of a cancerous diathesis, intensified by the irritation of an operation? Or that which presumes that in the operation germs were set free, were carried by the lymphatics or veins, or in the areolar tissue, and becoming arrested here and there, took root, as it were, and were developed into new tumours? The patient speedily died from the effects of the disease.

It is a fact, too, which is worthy of note, that while primary cancer is uniformly single, secondary cancer is most frequently multiple. It is not a little strange that Rokitansky cites this as a proof of the existence of a cancer "dyscrasia." He admits that "carcinomata originate and subsist not rarely as local evils;" but he adds: "Far more commonly, however, they are associated with a dyscrasis, which in point of fact, often precedes and engenders the cancer. *Hence* the multiple appearance of carcinoma, as a sequel to a single one—as the sequel to the extirpation of a voluminous and hitherto solitary one. Hence, in other cases, the original appearance of cancer in several organs simultaneously, or in rapid succession."

This *original* appearance of cancer in several organs simultaneously can never be proved. But surely the multiple appearance of the disease after the development or the extirpation of a single tumour points to the setting free and dissemination of germs rather than to a "dyscrasis." And a little further on he states himself that "the highest grades of cancer crasis originate through infection—that is, through the reception into the lymphatics, or more especially into the bloodvessels of cancer cells, or of cancer blastema, of a lax, soft, semifluid character." And these cancer-cells and blastema are derived from an original tumour, which has ulcerated, or has penetrated into the canals of the bloodvessels.

If the highest grades of cancer crasis arise from infection from an original tumour, and are indicated by the production of multiple growths, why assume a dyscrasis which precedes and engenders the original tumour?

Nothing, in fact, can more resemble the secondary occurrence of cancer than the deposits and consequent secondary formations in purulent infection. The mode of distribution of the formations in the lungs and liver are at times so like, that they might, at first view, be mistaken for one another, dispersed here and there in equal sized nodules; in one or two places, perhaps, larger deposits formed. And when we find the same tendency to multiple reproduction in the neighbourhood of an original growth, whether after operation or not, why, in the face of the fact which Van der Kolf pointed out, and which is now established, refer it to any pre-existing constitutional cause, which even a *constitutionalist* would hardly admit in the case of these lung and liver deposits, and which he would certainly deny in similar local deposits from purulent infection? My own conviction is, that in most cases of recurrence, where the tumour has even been to all appearance entirely removed, the recurrence has been due to the presence of minute cancer elements which have escaped the knife, or, as is perhaps as frequently the case, to germs being set free by section of the tumour or of diseased tissue around it during the operation, and their implantation into the newly-cut structures. That this is the case is, I think, shown by the fact that where a voluminous breast with only a small tumour embedded in it is removed, and the incision is consequently very long in proportion to the size of the tumour, the recurrence frequently takes place along the whole line of cicatrix, not especially in that part which corresponds to the site of the tumour. If, then, the view which has been so ably advocated by Mr. Moore be correct, that cancer is a local disease which becomes disseminated from the point of its first invasion, the practice of early extirpation cannot be too much insisted on. A tumour, as soon as it is recognised as cancerous, should be removed, if practicable, at once. A few days, even, may make a difference; not, perhaps, to any extent in the size of the tumour, but in the extension of its germs beyond its apparent limits. It would be better even to extirpate doubtful tumours, some of which might turn out on after examination not to be cancerous, than to leave them to develope themselves, and then to find, too late, perhaps, for help, the characters of malignancy becoming marked. These views, which had often been discussed amongst the members of the surgical staff at the Middlesex, encouraged Mr. Moore, in a severe case of breast cancer, to apply the solid chloride of zinc to a large portion of the surface of the wound made in its extirpation, as he had effectually done in a case of extensive epithelioma of the face, which he showed at one of the meetings of the British Medical Association in London. This was done in April, 1864.

The patient was a spare, healthy woman, forty-three years of age. The right breast was the seat of a small cancer, with adherent skin, cherry coloured over the centre of the tumour. In the axilla was a cluster of enlarged glands, which formed a tumour a little larger than that in the breast. The mammary disease was only known to have existed for four months. Mr. Moore first detached all the axillary glands, and tying them above, cut them off. He immediately touched the stump with the solid chloride of zinc. He then dissected out the diseased mammary gland, and before closing the wound he lightly touched nearly all the exposed axillary tissue with the chloride. The case ultimately did well, but what was remarkable was, while the skin over the breast sloughed to a slight extent, and was inflamed for some distance, due in great measure, according to Mr. Moore's notes, to its being somewhat tightly held together by the sutures, there "was no tension or great discharge from the axilla," and after a fortnight there is a report that the axilla is well filled up. It was clear that, at any rate, the touch with the caustic did not retard the cure.

To me it occurred that one might obtain the benefits which were sought by using the caustic in a less active form, and that a strong lotion of the chloride of zinc applied freely over the whole exposed surface, after an operation for the removal of cancer, would penetrate to some little extent beyond the limits of the section, and would at least destroy any floating particles of the disease which might adhere to it without endangering the vitality of the whole thickness of the flap.

The first case in which I tried this plan was that of a lady, forty-one years of age, who had a cancerous tumour in the right breast. She was well nourished and healthy, and had noticed the tumour about a twelvemonth. In the axilla was a gland slightly enlarged, but not hard, and in all respects the case was a favourable one for operation. The operation was performed in March, 1865. The strength of the solution employed was twenty grains of the chloride of zinc to the ounce of water; the whole surface of the wound was well sponged with it. The blood which still oozed was, as usual, rendered of a bright pink colour, and the contact of the solution at once caused a more free oozing from the exposed surfaces; otherwise no effect was perceptible. The lotion was thoroughly pressed in with the sponge, and in a little time the surface became soft and creamy in feel, and this softness extended to a little depth —a line, perhaps. Here I stopped, not knowing how far I might venture without causing sloughing of the flaps of skin. My impression was that a superficial slough would form, and would be thrown off by degrees during the suppurating stage, which, I assumed, must of necessity ensue, and which, in fact, I rather desired. The edges of the wound were, nevertheless, put together with sutures, save at the outer part, which was left quite open, to allow of the free passage of the supposed inevitable pus. A compress was put over the wound to check the tendency to any further bleeding into the cavity. After recovering from the effects of the chloroform, she complained for two or three hours of smarting pain; not more, I think, than is usually felt, and from that time she was entirely free from any pain at all. I thought it probable that on removing the compress, about eighteen hours after, the parts would be found swollen and angry, although the pain had been so slight; but instead of that, the circumstance most noticeable, was the absence of even the usual amount of fulness. It was evident that action, instead of being increased, had been diminished, one might almost say arrested, by the application. The skin, even up to the cut edge, looked and felt exactly like the skin of the other breast. Blood, in much about the same quantity as is usually found after such an operation, had oozed from the wound; but it was pink and creamy in character, and what was especially remarkable was the entire absence of the peculiar odour which is generally found in blood which has been pent up beneath a compress for some hours. There was, in fact, no animal smell at all. But what most struck me in the progress of the case was the absence of suppuration. The whole line of incision united in the course of forty-eight hours, except just at the outer angle, which discharged a *very* small quantity of the same pink, creamy-looking fluid for a day or two more, and then healed. The same absence of animal odour was noticed to the end. I certainly never saw a wound which did not heal absolutely by the first intention go through its process of cure so speedily or so quietly. The patient remains perfectly well to the present time.

In cases of cancer on which I have subsequently operated I have used stronger lotions. In this first case the strength was 20 grains to the ounce. I next tried 30 grains, and then 40 to the ounce. With the stronger lotion a more rapid effect is produced, and the blood exudes more abundantly, but this is only for a few seconds, otherwise much the same course of events has been seen as in the first case during the early period. Some have healed in the same rapid manner, in some there has occurred an after suppuration, but in none have any bad effects been seen.

What the effect of this treatment may be in limiting the tendency to return after extirpation of cancer, can only be determined by time and numbers. If the views of the diffusion and transplantation of cancer germs before expressed have any truth, it cannot but be beneficial; for to some extent it certainly must alter the character of the exuded matters and of the remaining tissues. My own impression is, that in cancer it would be well to go beyond the point hitherto reached, and that this may be safely done, for the effect of the chloride of zinc seems to be limited to the point with which it is brought into contact. It appears to produce very little irritation beyond that point, so that it may be worked into the inner surface of the flap till the tissue is softened to within a few lines of the surface without risk to the vitality of the remainder. In the course of the last summer I removed a large cystic tumour from the breast of a lady. The skin was stretched over it, so that when the tumour was away, there remained two extensive flaps of thinned skin with scarce a trace of adipose tissue. I was almost afraid to use the lotion to these flaps, but I did so until the inner surface was softened. The whole was healed in about twelve days, without anything that could be called suppuration, and with the same quiet inactive state of the skin as I have before described. So far as I have observed, I am well content with the *special* effect of the treatment, though the cases on which I have tried it are too few and too recent to justify any opinion being formed. In three cases of breast cancer which have been operated on for more than six months there is as yet no appearance of return. In one—a case in which the disease made such rapid progress that it had increased by at least a quarter of its size in a fortnight—where it extended beneath the pectoral muscle and ran close upon the axillary vessels, it reappeared in the axilla in six weeks. In this case, however, the disease could not be fairly removed, and I was obliged to content myself with detaching as towards the depths of the axilla, throwing a ligature tightly round the neck and cutting it off. The operation was done in June last, and the improvement in general health and alleviation of pain which followed the operation fully justified its performance.

Another case was one of recurrent cancer in the breast a year and a half after removal. At the sternal part the disease was fixed to that bone. Towards the axilla there was a subcutaneous mass of the size of a small walnut. There were small subcutaneous tumours in the line of the cicatrix, but no deposits in the skin itself. The patient's general health was perfectly good; the axilla was free. I removed the whole of the disease, using the strong chloride of zinc lotion to all the soft parts, and destroying the sternal part, after cutting the disease away, with the actual cautery, and laying over this chloride of zinc paste; I treated the part, in fact, precisely in the same way as has been so successfully adopted at the Middlesex Hospital, since Mr. Moore tried it in the case of cancer of the face before referred to. All the parts healed rapidly and with scarcely any suppuration, except, of course, at the sternal part. She was at the end of six weeks seized with symptoms of pyæmia and died. The origin of this was clearly in the cancellous structure of the sternum; but on the most careful examination of all the tissues about the seat of operation, no trace of cancer could be found. The final result of this case may seem to invalidate the statement which has been just made, that no bad result has attended the use of the chloride; but it does not affect the question. The pyæmia took place long after all direct effect of the chloride had passed off. The bone was intentionally exposed to some extent below its surface to ensure the removal of all cancer germs which might have developed in it, and was liable ultimately to any accident which such an exposure might subject it to. My mistake here was in not using from time to time a weak solution of the chloride, so as to keep in an undecomposed and healthy state the matters which lodged in the cancelli.

From data so limited it would be absurd to form conclusions. I am myself so satisfied with the results at present obtained, that I trust the plan will be far more extensively tried.

The singularly favourable way in which the wound healed in the first case on which I tried this plan, satisfied me that it might be beneficially adopted in other than cancerous cases. There was one point which especially struck me, as giving great value in hospital practice; the perfect purity of the discharges from the wound during the first few days after an operation. It is well known that the presence of decomposing animal matter tends to bring any dead animal matter with which it may be in contact into a rapid state of decomposition; and if this take place in a wound, it will certainly interfere for a time with the natural and healthy processes of cure, and may induce erysipelas or pyæmia.

That this decomposition does usually occur is evidenced by the peculiar sickly animal smell which is perceived whenever a wound which has been covered for a few hours is opened. When, on the contrary, a wound has been fairly impregnated with the chloride lotion, there is invariably an absence of any animal smell whatever for two or three days; and, unless some diseased tissue remain in the wound, there may be none throughout the healing. Were this the only advantage, it would be a great one; I believe that in our hospital it has saved many a patient from erysipelas: certainly we have been for the last eight months very free from it after operations, while just before it was very prevalent. But this may be an accidental coincidence merely, and time and experience can alone determine how much is due to the treatment. It is not, however, the only advantage. One of the most striking consequences of the application is the quiescence of the wound. The action which one would imagine must of necessity follow the application of an escharotic so powerful as the chloride of zinc, is never to be seen. The parts, up to the very edges of the wound, retain their natural colour during the early periods after an operation. I can state this confidently after the use of the lotion in varieties of operations, the removal of tumour, amputations, even with extensive and thin flaps, as Syme's and Mackenzie's amputation, operations about the rectum, involving the mucous membrane, and in the perineum, and in many others, as well as after accidental wounds. In many cases the wounds have healed in twenty-four hours, without the least fulness or swelling, and leaving a line of cicatrix which after a short time could hardly be seen or felt.

Some cases are at this moment under my observation. A week ago, my colleague, Mr. Shaw, performed two operations. One of these was on a strumous subject who had sinuses in the neighbourhood of the heel, running towards one spot. No bone could be felt, but it was thought that there must be some caries keeping up these sinuses. The heel was laid open to its full extent from before backwards through the track of one of these sinuses, but no disease of the bone could be found. The wound was well bathed with a solution of twenty grains to the ounce, and healed by the first intention without pain, swelling, or discoloration.

Another case was one of necrosis of the lower end of the tibia, with sinuses on the outer and inner side of the ankle. On laying open the outer sinus the joint was found to be opened, and the astragalus in part rough and denuded of cartilage, which still covered some portion of the articular surface. In the situation of the lower end of the tibia was a large cavity, communicating with the medullary cavity, in which lay several pieces of necrosed bone. The articular surface was gone. A portion of the shell of this cavity was removed, and the necrosed bone taken away. Here, by Mr. Shaw's permission, I used the forty grain solution, sponging out thoroughly and repeatedly with it the cavity of the bone and all the tissues surrounding the ankle-joint: up to the present time the parts have been perfectly free from swelling, pain, or redness. There has been scarcely any discharge, and what there is is entirely free from smell. The tissues about the inner ankle, which were very much infiltrated, and more than an inch thick, were also laid open, and gaped widely. They were put together with sutures; they did not unite, but were found next day to be covered with a thin layer of slough, which speedily peeled off and left a healthy granulating surface below. There has been throughout an entire absence of all inflammatory action. In such a case as this, there would, I believe, have certainly been a foul discharge for a little time at least after the operation, had the lotion not been used. The tendency to form a superficial layer of slough is seen in every case in which the skin is thus infiltrated from long-continued disease. It is due, probably, to this low state of vitality of the part. The slough separates very quickly, a healthy granulation covers the surface, and there is no inflammation of the adjacent texture. Another and very striking case is now under the care of Mr. Nunn. He removed the head of the femur below the trochanters from a woman with caries of the hip. She is suffering also from considerable disease of the liver, and her general health is much impaired. The acetabulum was perforated, and the finger could be passed into a cavity beyond; but there was no carious bone to be felt. The lotion was very freely used. The operation has only been done three weeks, but the parts are quite covered with healthy granulation, and the discharge has almost entirely ceased. But an unhealthy inflammation has taken place in the opposite groin, over a large resonant swelling, which appears to be connected with some perforation of the bowel. In this case the lotion was injected for some days into the cavity caused by the operation.

This singular absence of action in the wound is, as one might suppose, accompanied by absence of pain. That great pain sometimes attends the immediate application to a sensitive wound need not be mentioned. But this is not lasting. In most cases it subsides in from one to two hours; in some cases it does not occur at all. Where the application is made after an operation done under chloroform, the patient generally remains altogether free from pain. This is particularly the case when morphia has been subcutaneously injected, as Mr. Moore first suggested, immediately the operation is concluded, and before the effects of the chloroform have passed off. After this proceeding the patient often remains calmly asleep for some hours, and wakes entirely free from pain. There seems to be also less tendency to sickness. When, however, the immediate pain of the application has once subsided, the comfort which the patient enjoys is very striking. I have seen cases in which the patient could not tell from their sensations after a couple of hours that any operation had been done. One can explain, perhaps, from observing the action of the chloride, why action and consequent pain should be lessened or altogether prevented. It is quite clear that the chloride of zinc does not act as an irritant beyond the point of contact. If its use is carried so far as to produce an eschar, the eschar will act as an irritating body, and there will be inflammation and swelling around it; but if applied short of this, and it requires a continued application of the solid chloride to make an eschar in the natural structures, it produces a peculiar pulpy state of tissue, widely different no doubt from the natural tissue; but certainly not eschar, not a charred mass which *must* be removed by the ordinary process of separation below and around it. Were the surface of a large wound converted into an eschar, it could not heal in twenty-four hours. And yet the tissue appears disintegrated; it is rapidly discharged as a creamy exudation, leaving the parts below perfectly natural in appearance. This can easily be seen when the lotion is applied to an open wound. In this creamy surface must be involved all the sensitive and vasculo-motor nerves, and their function must be arrested. With many escharotics, as the actual cautery, the acids, &c., the irritating effect is propagated along the nerves beyond the point of contact. Although the nerves exposed on the surface may be destroyed, the effect is carried beyond this point, increased nerve action and inflammation is the result. It is not so with the chloride of zinc. From whatever cause it may be, the action terminates at the point of contact. The sensitive surface is destroyed, but no irritation is set up beyond the part directly acted upon. Hence there is no pain, no vascular action, no inflammation. Whether this is or is not the true explanation, the fact certainly is as I have stated it.

Of course I do not mean to assert that all this immunity will be found in every case; but I can safely say that I have never seen it otherwise, and that, as a rule, I have never seen the general run of cases go through so favourable a course as since I used this application.

The mode of application has been described in connexion with the first case in which I tried the solution. The first effect is always to stimulate the small vessels and cause a general oozing of blood from surfaces which had been previously dry. The blood becomes pink and creamy by contact with the chloride. On further application of the solution the whole surface is softened and assumes the same pink colour. The blood will continue to ooze out as long as the solution is applied, and for a short time after. Every part should be well saturated with the chloride—the edges of the skin, the adipose tissue, the spaces between the muscles, the medullary cavity, or cancellated structure of bone. At first I was afraid to touch bone with it, but I find now that no harm comes of doing so. The surgeon need not hesitate to apply it even to thin and delicate structures. Those cases have done best in which it has been most thoroughly used. For wounded surfaces I generally use a solution of from thirty to forty grains to the ounce of water.

An objection has been raised to the use of the chloride that union by the first intention must be prevented, and that we are therefore throwing away a chance of obtaining the best and safest mode of cure after operation. Entire union by the first intention is, however, rarely, if ever, seen after operations of any magnitude. Parts of the wound may so heal; and were the objection valid at all, it would be simply on a question of more or less. Supposing, moreover, that in one case in twenty entire union by the first intention did take place (I am still speaking, of course, of large operations), surely it would be well to forego the chances of that one, if we could ensure the safer and better healing of the remaining nineteen.

The objection, however, is really not valid. A wound will heal entirely in twenty-four hours when the solution has been freely used. It may do so in less time, possibly, but I have certainly found it healed at the end of that time. And, as a rule, I have found wounds heal more rapidly since I have used the application. After what I have seen of its effect, I should have no hesitation in using it in a plastic operation in which, of all cases, one most desires kind and early healing.

Surgeons have, as is well known, tried various applications in the hope of preventing or lessening suppuration, or of warding off the evils which produce the large proportion of deaths after operations, such as erysipelas, pyæmia, and osteo-myelitis. Of these applications, the principal are strong alcohol, either pure, or holding in solution astringent or aromatic substances, or iodine. Alcohol and some of the aromatic tinctures I have tried; but though they act very beneficially in unhealthy wounds, I have not found that they protect recent ones for any length of time from unhealthy action; nor do they seem to lessen inflammation or promote union. Aloes enjoyed a very high reputation for many centuries for its healing qualities. When applied externally, it was said to hasten cicatrization and repress hemorrhage. I have often used it in the form of the compound tincture both to recent wounds and to old ulcers. In the latter, and especially in bad sores, it often does great good; but I never found any special benefit from its use in recent wounds. So long as it remains in contact with the wound, it prevents decomposition—as the spirit itself would—and it perhaps has a locally tonic effect on the tissues, but it requires frequent renewal, which could not be managed in a wound which it is desired to heal rapidly. The application of tincture of iodine in recent wounds was recently proposed by M. Pétrequin. Finding that suppuration was never present when this fluid had been injected into cavities or into the tissues, he recommended and practised its application to wounds made in the removal of tumours, especially when situated in the face or neck, where it is very desirable to prevent the formation of scars. Whether it has been tried by others I do not know; but for many years I have been in the habit of adopting a treatment which was suggested to me by Mr. Hoffman, late of Margate, the introduction of small tents covered with the iodide of starch into strumous glands in the neck, even before suppuration has occurred in them, the effect has been very satisfactory; but considerable pain and inflammatory action have followed the treatment. Mr. Higginbottom has long recommended the application of a solution of nitrate of silver in recent wounds as well as in erysipelas, as a means of checking any tendency to suppuration. I have not tried it, nor do I know whether it has been generally useful. I doubt, from what I have seen of its action in slight wounds, whether nitrate of silver could be applied as freely as the chloride of zinc; or whether, if so used, it would preserve the parts in a healthy state for so long a time.

We find, too, that Mr. Butcher advises the use of the actual cautery in some operations. He writes: "I have found its application in this way"—quickly brushed over the surface—"most serviceable after excision of the upper jaw, and other severe measures, in arresting unhealthy forms of inflammation, diffuse or erysipelatous, and arousing at once a reparative inflammation."

I have met with nothing which acts in so unmistakably beneficial a manner as the chloride of zinc.

But there is nothing new under the sun. My colleague, Mr. Shaw, was speaking recently to Dr. Brown, of Sloane-street, on the subject of the use at the Middlesex of the chloride lotion, who told him that the subject was not new to him, as he had in early life seen the use of Burnett's fluid in recent wounds. Dr. Brown has kindly favoured me with the following account:

"When I was a young disciple of that most worthy chief of the medical department of Portsmouth Dockyard, James Henderson, a tank was erected in the ropery for saturating yarns, &c., in Sir William Burnett's fluid. Now, it was the duty of every man in the whole establishment to come and report at the surgery the slightest hurt he might receive in the course of his work; but soon after this tank was set up we found on several occasions that men in the ropery meeting with slight wounds and abrasions did not so report themselves, and upon being remonstrated with, they said they had gone directly to the tank and applied some 'solution' to the part, and it always got well very quickly. Practically, the men employed at the tank found if they had cuts about their fingers that they healed surely and speedily through dabbling in the solution, and the fact soon spread amongst their fellows. This presently so attracted the attention of my excellent and observant friend, that we had Sir W. Burnett's solution brought into the surgery to be used in surgical treatment, and I am almost sure that at some time between 1842 and 1844 it was specially referred to in a Report to the Director-General of the Navy."

The Director-General of the Navy was so good as to have the reports of these years inspected; but he informs me that no mention of the external use of Burnett's fluid is to be found in them.

[The value of chloride of zinc is further confirmed by Mr. Paget, in the following cases at St. Bartholomew's Hospital.]

G. S., aged 55, was admitted March 31st, 1866, with an epithelial growth over and adherent to the right malar bone, and involving the structures of the cheek. The disease commenced seven years ago as a wart, which was cut away, and twelve months ago, when it had attained a considerable size, it was again cut away, and nitric acid applied. On admission the growth was as large as a five-shilling piece, irregular in form, ulcerating, very hard and immovable. The skin around was involved in the growth, and tuberculated. But little pain or inconvenience was experienced by the patient.

On April 7th, Mr. Paget removed the growth, together with a portion of the malar bone. There was considerable hemorrhage. A solution of chloride of zinc (thirty grains to the ounce) was mopped freely into the wound, and produced the characteristic creamy pink colour. A piece of dry lint was placed over the surface. The man suffered no pain in the wound as a result of this application.

10th. Wound was dressed. On removing the lint there was a moderate amount of suppuration, and not the slightest odour was perceptible. From this period the progress of the case was uninterruptedly favourable.

We saw the man on April 18th. The wound presented a healthy-looking granulating surface, with moderate suppuration, and a few portions of sloughing tendinous fibre. There was not the slightest trace of any smell.

C. C., an Irishwoman, aged 65, presented herself at the hospital on March 3rd, with a compound dislocation of the lower end of the right ulna and fracture of the lower end of the radius. Reduction was accomplished, and the limb placed upon a splint. The patient was advised to come into the hospital, but this she refused. She went away, and did not show herself for a week.

On March 10th she was admitted. Phlegmonous erysipelas had taken place, and her arm was in a very unsatisfactory condition. Sloughing of the tendinous structures ensued, the wrist-joint became the seat of abscesses, and such general disorganization of tissues with depression of vital powers took place that, about a fortnight afterwards, Mr. Paget judged it expedient to remove the limb. To this the old woman would not consent.

Her condition daily became more unsatisfactory, and on April 7th she at last gave permission for the operation to be performed. The arm was then amputated just above the elbow-joint. The structures were very œdematous and indurated. A solution of chloride of zinc (thirty grains to the ounce) was applied somewhat sparingly to the wound. The change in colour, Mr. Bloxam informed us, was not so well marked in this as in the preceding case, where a freer application was made. Dry lint and a loose bandage were applied to the stump. She suffered no pain from the solution.

Next day she was doing very satisfactorily. There was fair reaction; a soft skin; pulse 90. No odour was perceptible about the wound. On the following day a slight odour was observed.

April 10. The dressing was removed. There was a fair amount of discharge somewhat offensive in smell, but the stump otherwise was looking well.

12th. There was a good deal of discharge from the wound. The sutures, which were beginning to cut their way out, were removed. The wound was syringed with water. The entire surface was apparently undergoing sloughing.

We saw this patient on the 18th. The sloughs were then separating, and the lower portion of the wound was granulating. The stump was shrinking. There was still some odour about it. The general condition of the patient was satisfactory.

On the 28th we learnt that the wound was nearly healed.

The occurrence of sloughing in the latter of these cases was to be expected under the very unfavourable condition which the patient presented. Mr. De Morgan remarks in his paper; "The tendency to form a superficial layer of slough is seen in every case in which the skin is infiltrated from long-continued disease. It is due probably to the low state of vitality of the part. The slough separates very quickly, a healthy granulation covers the surface, and there is no inflammation of the adjacent textures." From our observation of cases in the Middlesex Hospital, we think that it might have been better, notwithstanding the woman's low state, to have used the lotion a little more freely. The fœtor would in all probability have been longer delayed, and there would have been no greater amount of sloughing.—*British and Foreign Medico-Chirurgical Review, Jan.* 1866, *p.* 201, *and Lancet, May* 5, 1866, *p.* 483.

46.—FORCIBLE FLEXION OF THE KNEE FOR ANCHYLOSIS IN THE STRAIGHT POSITION.

By Dr. PHILIP CRAMPTON SMYLY, Surgeon to the Meath Hospital and County of Dublin Infirmary, &c.

[The patient had suffered from a violent attack of rheumatic arthritis some years before. The limb had been fixed in the straight position, and had become perfectly anchylosed. There was not the slightest motion in any direction, and the patella was firmly attached. There was complete absence of pain and of any trace of inflammatory action.]

Miss W. complained bitterly of the constant source of vexation her stiff knee was to her. She could not walk any distance on account of the fatigue caused by swinging the limb round to place it in front of the other; she could not sit down with comfort except on a very high seat; she could not kneel; and she could get up stairs only by "putting her best foot foremost," and dragging the stiff one after her.

Under these circumstances I advised her to have the joint forcibly flexed, and then to employ passive motion.

I first bandaged two strong iron splints, well padded with cotton wool, to the thigh—one in front and one behind—extending from about half-an-inch above the point I wished to effect the fracture to the groin in front, and as far behind. This was to avoid the danger of breaking the thigh bone instead of the knee. Miss W. was fully chloroformed and drawn down on the table, so that the end of the posterior iron splint rested exactly on the edge. Mr. Collis with both hands propped firmly on the anterior splint.

I grasped the foot with my right and placed my left hand about the middle of the tibia. By throwing the whole weight of my body on my left hand I succeeded in breaking through some of the adhesions. This process had to be repeated three times before the knee could be bent to a right angle. Each time the adhesions gave way with a loud crunching sound. The thigh splints were then removed, and the limb placed in a long jointed splint, with a screw behind, and lightly bandaged—the leg and thigh being left at an angle of about forty-five degrees. The knee was kept constantly iced for forty-eight hours, and the foot kept warm.

Every day the position was slightly changed, so that towards the end of the week the limb was again straight. The screw was then slowly tightened, several times a day, until, in about three days, the leg and thigh were nearly at a right angle. For about six weeks this gradual process of flexion and extension was persevered in ; then the splint was removed, and Miss W. was allowed to get up. She was desired, always, to sit on a low chair and to try and strain the foot backwards so as to touch the leg of the chair.

Miss W. then returned to the country and has enjoyed perfect health ever since.

[Two years after the operation the report of the case is as follows :]

"She has as good use of that knee as of the other—can take long walks, dance, run, or exercise in every way as freely as before the joint became affected."—*Dublin Quarterly Journal, Feb.* 1866, *p.* 18.

47.—SUBACUTE SYNOVITIS AND CONTRACTED JOINTS.

Mr. W. ADAMS has been employing for some time past at the Great Northern Hospital a mode of treatment for effusion into joints which he finds very satisfactory. In subacute synovitis, especially of the knee-joint, where there is much fluid, a large blister is applied to the skin, and when this has risen, the cuticle is cut away completely, leaving a raw surface, which is then dressed with strong mercurial ointment. The dressing is continued so long as the blistered surface remains unhealed. Occasionally healing takes place very rapidly. In such cases a dressing of equal parts of mercurial and of savin ointment is applied in order to keep the blister open. No constitutional disturbance or salivation, Mr. Adams tells us, results from this treatment ; but he finds the fluid in a joint absorbed with unusual rapidity. The best cases for the employment of this method are those where, after an attack of acute synovitis, although all inflammatory symptoms have subsided, the amount of effusion shows no tendency to diminish. In very chronic cases with much thickening, he considers Scott's dressing more useful. As regards the propriety of employing forcible extension in the contraction which so often follows synovitis, Mr. Adams considers that where this condition ensues upon traumatic or rheumatic affection of the joint, the process is allowable, and may often be employed with great advantage. His plan is to give chloroform, and apply a fair amount of force, not in jerks, but steadily. When the joint is felt to yield, even ever so little, the limb is immediately placed upon a splint, provided with power of extension by screws, which are turned daily. In contraction from strumous disease of joints, he objects even to this amount of violence, having frequently seen grave mischief lighted up by this means. When, however, a joint has been very long quiet, and bony anchylosis has taken place, the strumous taint meanwhile almost disappearing, the objection ceases to apply with the same force, and very careful extension is often found useful.—*Lancet, Jan.* 6, 1866, *p.* 8.

48.—ON AN EASY METHOD OF REDUCING ORDINARY DISLOCATIONS OF THE SHOULDER JOINT.

By Wm. Elliott Porter, Esq., Linfield, Sussex.

While a student at the London Hospital a dislocated shoulder was brought in, which great force failed to reduce. On pausing for a short time before adopting other means, one of my fellow students picked up the arm and gently manipulated it; while doing so the bone slipped into its place—how, none could say, but the dislocation was reduced. I thought of it a great deal, and came to the conclusion that the humerus, without the exhibition of great force, was a sufficiently powerful lever, when used as one to reduce an ordinary dislocation of the shoulder. Not long after I left the Hospital, and had to wait for some time before I had an opportunity of testing my plan.

While acting as House-Surgeon to the Dorset County Hospital in 1856, a dislocated shoulder was brought in; it was a stock case, that is, it came frequently; but it must have been troublesome to reduce, as they always used pulleys for it, and the Hospital porter began getting them in order; I thought, however, I would try my plan first.

The patient was a moderately muscular man, at about the middle period of life, and having undressed him I laid him on a mattress on the uninjured side; kneeling behind him, I placed the palm of one hand firmly on the head of the scapula and the fingers in the axilla under the head of the humerus; with the other hand I rasped the condyles and pressed the arm to the side, then drew it backwards and thus got a better hold with the fingers engaged at the head of the bone ; by the next movement I slid the arm forwards and depressed the elbow, at the same time rotating outwards, and lifting the head of the bone it immediately slipped into its place.

Since I have been in practice I have tried the plan three times on healthy, muscular young men, all first dislocations ; in two I succeeded quickly and easily, and failed in one which had been done three days ; the old way of the heel in the axilla was in this case after some difficulty successful.—*Med. Times and Gazette, March* 10, 1866, *p.* 264.

49.—ON THE TREATMENT OF FRACTURE OF THE RADIUS AT THE STYLOID PROCESS BY MEANS OF GORDON'S SPLINT.

By Lawson Tait, Esq.

[Few fractures have had so many ingenious splints devised for their treatment as that known as Colles's fracture, yet most or all have been given up, and the ordinary palmar and dorsal straight splints generally used, although frequently with unsatisfactory results.]

In spite of the utmost care most cases of this fracture turn out unsatisfactorily, and many are the actions of damages that have been raised on its account. The reason of this non-success is, I think, very plain ; let any one examine his own wrist, and the following explanation will be clear. Holding the hand straight out in a plane with the forearm, it will be seen that, while the dorsal aspect is almost a straight line, there is a considerable concavity at the wrist on the palmar aspect ; indeed, that a line drawn from the elbow to the ball of the thumb would be, so to speak, the chord of a segment of a circle. Thus it is that when an arm, with the radius broken as it is in Colles's fracture, is pressed by two straight splints, one on either aspect, extending from the elbow to the fingers, the upper fragment must necessarily be pressed towards the palmar aspect of the limb ; while the lower fragment, which is practically the same in this condition as the ball of the thumb, is pressed in the opposite direction—in fact, that the distortion is only increased by the splints, as they press the fragments in the very direction in which they are already displaced. If this be correct, then it is easy to understand the success which has attended the use of Dr. Gordon's splint in the treatment of this fracture, and to believe that is devised on sound anatomical and mechanical principles—that it really is what all splints ought to be, viz., a dermal skeleton.

This instrument was originally invented and described by Dr. Gordon, of Belfast, the only notice, however, which I am aware that it has subsequently received is in a paper by Mr. Stokes in the Dublin Medical Journal. It is composed of two pieces of wood, the one for the palmar aspect of the forearm being about nine inches long, two and a quarter inches wide at the wrist, and three and a half wide at the elbow; the surface to be in contact with the skin is slightly hollowed out to fit the arm, and along its radial border it has screwed to it a wooden bar or pad, which is rounded off at the distal extremity to fit the concavity of the radius; this latter, of course, necessitates that, to fulfil this condition, separate splints are required for the right and left arms.

The pad, in addition to its being rounded off at the extremity, is rounded all along its inner surface so as to press accurately against the radius throughout nearly its whole length, and it is of sufficient height to embrace rather more than half the thickness of the forearm. The other portion of the apparatus consists of a plain piece of three-eighth inch board, two inches and a quarter broad, and two inches longer than its fellow; it is for application to the dorsal aspect of the forearm, and has the surface to be in contact with the skin slightly hollowed, and it likewise has its distal extremity transversely rounded. Its application is effected as follows:—The fracture having been reduced, the limb is retained in position by an assistant, the lower part of the apparatus is then applied, padded with spongio-piline or lint, to the radial portion of the forearm alone, and not to the hand. Then the upper splint is to be applied, likewise padded, in such a manner that the proximal ends of the two parts of the apparatus are maintained at the same level, while the distal end of the upper one projects about two inches beyond the end of the radius. For a more particular description and a drawing, see Dublin Medical Journal, for February, 1865. The whole apparatus is firmly secured by two small straps with buckles. In this manner no pressure is exerted on either of the fragments but what is calculated to keep them in their correct position. The arm, during the after progress of the case, is recommended to be kept in the position most agreeable to the patient, which will be found to be that of almost complete pronation. In the employment of this apparatus the wrist will be found to be confined only to a limited extent, while the movements of the fingers and carpo-metacarpal articulations are quite unimpeded; thus entirely doing away with the most objectionable condition of stiff joints, which is such an annoyance both to surgeon and patient for weeks after the common splints have been removed from the forearm.

Shortly before I became acquainted with this splint, I met with two cases of Colles's fracture, in which, in spite of the greatest care I could possibly bestow on them, there still resulted a considerable degree of the deformity peculiar to this particular injury. The first case occcurred in an old lady, and the other in a young collier lad ; in both the injury resulted from a fall on the palm of the hand. The unsatisfactory results of the ordinary methods of treatment of this fracture having been thus prominently brought under my notice, I was induced to pay special attention to what had been suggested by surgical authors as to the cause of the displacement, and as to the best means of overcoming the resulting deformity. Much has been written on the action of those muscles which some have supposed to be the cause of this peculiar deformity ; but I think that this is one of the many instances where muscular action is blamed for ill effects of which it is blameless. The deformity seems to me to be caused solely by the direction of the violence which is the cause of the injury, this being almost invariably a fall on the palm of the hand. The mechanism of the fracture seems to be that when the patient falls on the hand, and that by its being forced outwards it drags with it the apophysis of the radius, the ligaments breaking the bone rather than yielding themselves. Much the same, indeed, as in the very analogous fracture of the fibula at its lower fourth, where the internal lateral ligaments of the tarsus much more frequently drag away with them the tip of the malleolus externus than are ruptured themselves. Again, the idea that the displacement depends merely on the violence is borne out by the occasional occurrence of a case where the patient falls on the back of the hand, and when the ball of the thumb and lower fragment of the radius are driven upwards and towards the palmar aspect of the forearm. Bearing these things in mind, and looking at the great improbability of fragments so displaced returning spontaneously to their normal position, it seems to me that, in this instance at least, the theories which would attribute to particular muscles the power of drawing particular fragments this or that way, thus producing and maintaining the displacement, are quite needless. What is required in the treatment of this fracture, if we wish to obtain a perfect result, is not mere repose of the parts, which alone is secured by the ordinary straight splints, but such special adjustment of the normal curve of the shaft of the broken radius with its apophysis as will restore their normal relation to each other, and to the corresponding extremity of the ulna. This result theory satisfied me was obtainable by the use of Dr. Gordon's apparatus, and the result in the following cases, will, I think, satisfy any one of its utility and success.

Since reading Mr. Stokes's paper on this splint, I have met with two cases of the fracture, both of which were treated by means of it with remarkably pleasing results. The first occurred in the left arm of a boy, about eight years old, who fell from a height and lighted on the palms of his hands. In this case, the deformity was excessive, putting me in mind at the moment of that mysterious symbol connected with our early faith, known to antiquarians as the zig-zag sceptre ornament. He had his arm in the Gordon splints scarcely three weeks ; and now, from careful examination of both wrists, it could not be told in which arm the fracture had occurred. The other case occurred in a gentleman, aged 63, whose carriage was upset, and who likewise lighted on his palms. In this instance the deformity was well-marked, but not nearly to so great a degree as in the former example. After their original adjustment, the splints were not touched for six weeks, and there now exists not the least deformity. In fact I had an opportunity of examining this gentleman's wrist within the last few days, and am quite as well satisfied with it as with the other.

Recently Dr. Heron Watson mentioned to me a case of this fracture which he had treated by Dr. Gordon's apparatus with the most satisfactory results. The patient was an adult male, and had met with the accident in the usual way. "The injured limb," writes Dr. Watson, "had been put up in the first instance in Gooch's splints by my House-Surgeon ; but when I saw him next day, as he was uneasy, I took them off and applied Dr. Gordon's. They were adjusted two or three times while they were required, which was only four weeks. The original displacement was well marked, and the result was eminently satisfactory, the position of the ulna with reference to the carpus being natural, and the movements of the wrist quite unimpaired ; while the rotation backwards and outwards of the styloid process and articular surface of the radius was, if present, imperceptible."

In none of the cases did the patients complain of the slightest pain or inconvenience arising from the apparatus.—*Med. Times and Gazette, Feb.* 17, 1866, *p.* 170.

50.—ON SOME INJURIES TO THE HEAD AND NECK OF THE HUMERUS.

By JONATHAN HUTCHINSON, Esq., Surgeon to the London Hospital, and Lecturer on Surgery.

We will classify fractures of the humerus near to the shoulder-joint not into those at the surgical neck and those at the anatomical neck, but into those *below the tuberosities* and those *through the tuberosities.* Respecting the latter, I have now to ask your attention to a most important practical point, and one which you will find scarcely mentioned in books. A few months after a fracture through the tuberosities, or, what comes to the same thing, a separation of the epiphysis, it is very likely that all the appearances of a dislocation will be assumed. Owing to the altered form of the neck of the bone, the head is gradually displaced downwards, until a hollow is caused under the acromion, and the aspect of things is exactly like that of a dislocation. You may even feel the head of the bone in the axilla, though not so low down, nor so distinctly as in a true dislocation. It often happens that a patient in this stage of things consults another surgeon, and is told that a dislocation has been overlooked, or, still more frequently, a surgeon discovering this state in a patient whom he has himself treated, is led to the belief that he has made an almost disgraceful mistake. Sometimes this deceptive appearance is present from the first, but more usually it is produced when the patient begins to use his arm, and if present at first it always increases. I have seen this often, and have seen reduction attempted not unfrequently. A boy, aged 14, was brought to me by a surgeon, who believed that he had overlooked a dislocation. The deformity was very considerable—a hollow under the acromion, the head of the bone felt in the axilla, the fibres of the deltoid stretched. Two months had elapsed, and the surgeon who brought him expressed himself as certain that the symptoms now present were not there at first. I detected irregularity and thickening about the line of the epiphysis, and I found also that by pressing the arm straight upwards I almost filled up the subacromial hollow. I convinced myself and my friend that the accident had been a separation of the epiphysis, and that there was no reason to reflect upon the treatment. The most conclusive example of this condition, however, came under my notice nearly twenty years ago, when I was a student. Being at an autopsy on a lunatic who had died in asylum, I observed deformity in one shoulder, and remarked that it must be dislocated. The medical superintendent remarked that he had treated the man a year ago for fracture of the neck of the humerus, and that he felt confident that there was no dislocation. He willingly gave me permission to remove the bones, although expressing his fears that I should expose some bad surgery. We found on dissection that there was a fracture through the tuberosities, and the head of the bone had travelled down to a new articular facet just below the glenoid cavity. This specimen is, I believe, now in St Bartholomew's Hospital Museum.

When the appearances of dislocation are present in a case of fracture from the first, they are due to displacement inwards of the upper end of the lower fragment, and the higher the line of fracture the more deceptive they will be. At this stage, however, we have the symptom of crepitus to help us, and it is decisive. The hollow under the acromion is also never so definite as it becomes at a later stage. At the latter, when the bone is consolidated, it is often exceedingly difficult to form a decided opinion.

From this clinical fact two corollaries arise—first, that we should be exceedingly careful in giving opinions respecting old cases which have not been under our own care from the first; and secondly, that when we have to treat such a fracture we should clearly explain to the patient what the result will be. In one case recently in the hospital we had all the appearances of dislocation, with the most positive crepitus in the neck of the bone. At the end of a month, when the bone was united and the swelling had disappeared, we had yet more deceptive appearances, and explained to the man that, should he ever be examined by another surgeon, he might very likely be told that a dislocation had been overlooked, and I offered him a written statement of what the nature of his injury had been. I may repeat that in the first stage the diagnosis is not difficult; the abnormal mobility, the crepitus, and the ease with which the elbow may be brought down to the side all point to fracture.

It is possible that dislocation and fracture may occur simultaneously. Two or three times I have suspected this complication, but I have never had an opportunity for proving it, or of examining a dissected specimen. It is *à priori* very improbable that such lesions should be produced together. When the neck of the bone snaps by violence there is no longer any agency by which the head of the bone, now discontinuous with the shaft, should be displaced, and on the other hand, if once the bone has been dislocated it would be exceedingly difficult by any continued violence to break it. I suspect strongly, therefore, that most of the cases in which we hear of dislocation and fracture together, are really instances of fractures unusually high up, and in which, as I have just explained, the appearances of dislocation are subsequently simulated.

In any case of separation of the epiphysis in which the two fragments should be thoroughly liberated from each other, the epiphysis itself would in all probability be displaced by the three muscles attached to it (sub-scapularis, supra-spinatus, and infra-spinatus) in such a direction that its under surface, instead of looking downwards, would look directly outwards. There is nothing whatever to counteract these muscles. Should it occur, no surgical art could bring the epiphysis down again, and the shaft of the bone would unite to it at an angle. Thus the neck of the humerus would come to resemble in form the neck of the femur of a young subject, and subsequently the elevation of the arm by the deltoid would push the head of the bone lower and lower in the glenoid cavity, and gradually produce the appearance of dislocation to which I have adverted.

As regards the treatment of injuries to the neck of the humerus, we may note that in the majority of cases there is little or no displacement, and that the fragments are locked or impacted together. The higher up the injury and the thicker the bone at the site of fracture the more likely is this to occur. In such cases it matters little what plan of treatment is adopted. If there be displacement, it will probably be that of the lower fragment inwards, and to counteract this we must employ extension and put a large pad in the axilla. It may be sufficient in some cases to employ the plan of a large axillary pad with the arm bandaged across the chest. On the whole, however, I prefer to use a single long splint, well padded and pressed into the armpit, on which the arm and forearm may be extended with great efficiency. The only plan which would be likely to remedy the displacement which has been just described in reference to separation of the epiphysis would be by carrying the limb directly outwards at right angles with the patient's trunk. I have never tried this, and it would probably be very inconvenient in practice. In any case in which decided displacement existed despite extension in the straight, I should be inclined, in spite of the inconvenience, to try abduction of the arm, keeping the patient in bed and using plaster of Paris.

You will see that in discussing the treatment of these fractures we have to take notice quite as much of the extent of displacement as of the direction of the fracture. In many cases, whether of fracture through the tuberosities or separation of the epiphysis, there is only incomplete displacement of the fragments one from the other, and they still remain in apposition at parts. In such cases the fragments, by their mutual locking and pressure, prevent the muscles from effecting the characteristic distortion. The thicker any given bone is at the part where broken, the more likely it is that the displacement will be incomplete.

Then we must also recollect that in many cases the displacement will not be according to the direction of muscular action, but will be influenced rather by the position assumed by the bone at the moment that violence was applied. Very often the original displacement, although contrary to muscular action, is from some wedging of fragments, or more frequently from the intervention of tense bands of periosteum or fascia, never afterwards altered. Thus we know beforehand what the typical displacement is when the fragments are free to move under the influence of the muscles, but we cannot predicate of any given case what it will be, since it may be modified by other conditions. —*Medical Times and Gazette, March* 10, 1866, *p.* 247.

51.—ON A NEW FORM OF LEG-SUSPENDER AND BED-GUARD.

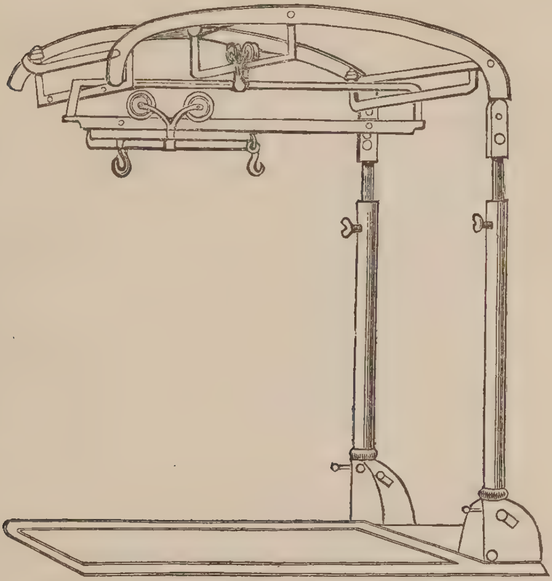
By Henry Greenway, Esq., Plymouth.

[An instrument which will alleviate the tediousness of confinement to bed, in cases of fracture of the lower extremity, must be received as a valuable addition to our armamentarium.]

In August, 1864, I left instructions at Weiss's for an instrument to be made, whereby I hoped to gain every movement that could be desired. These instructions were carried out, but my success was only partial. I have since made a number of experiments in order to attain the desired end, and I am happy in now being able to bring before the profession an invention whereby a patient is enabled to move his injured limb about the bed with nearly the same freedom as he would his sound one.

The accompanying engraving will give some idea of the nature of the apparatus. In general terms it may be described as consisting of a base or foot, two telescope pillars, and an overhanging top, to which is attached travelling gear, affording longitudinal, transverse, rotatory, and swinging movements. Also a contrivance attached to the base, whereby the pillars are supported, and any tendency to inclination corrected.

The *base*, which is placed on the battens of a bed, underneath the mattress, is a stiff iron frame, nearly square in form, at the outer angles of which are fixed *quadrant-shaped plates*. Attached to these are two *pillar stumps*, their lower, flattened ends being hinged to the plates, and their upper or free ends rounded so as to fit into the lower part of the tubular pillars. These stumps and plates not only support the pillars and parts above, but by means of screws, have the power of correcting any inclination—an occurrence which would happen if the battens of the bed, on which the base lay, dipped in the middle. (Only a very small, intermediate portion of the stumps can be seen in the sketch.) The *telescope pillars* are partly tubular and partly solid. Each tubular portion, at its lower end, fits on to the upper part of the pillar stump ; the solid portion of each pillar is attached to the upper framework by a joint which is fixed with a pin when the apparatus is in use. (This joint is placed for the purpose of portability, as by it the pillars can be folded underneath the top.) With the telescope arrangement the height of the apparatus can be regulated at pleasure, by means of a check-pin at the upper end of the tube. The *top*, which is supported by the pillars on its outer side only, is an iron framework of about the same dimensions as the base (nearly two feet square), which it overhangs. Attached to this framework is the *travelling-gear*, by which the various motions are obtained. The first portion of this gear consists of three flat bars placed longitudinally, and fastened to the top by their turned-up ends. The middle one of the three is the longitudinal truck-rail: the other two are bearing-bars. On the truck-rail are placed the trucks which allow the patient to move upwards or downwards in the bed. Passing through the holder of these trucks, underneath the rail, is an arm, the ends of which are turned downwards to support the transverse truck-rail. The ends of the horizontal portion of the arm rest on the bearing-bars, and consequently glide over them when the before-mentioned trucks are in motion. On the transverse rail run a pair of trucks similar to those above; these allow the limb to be moved sideways. Attached to the holder of these trucks, underneath their rail, is the rotary bar. This works on



a pivot, and enables the patient to direct the limb across the bed diagonally. On this bar are placed two hooks, from which suspension of the limb is made, and a swinging movement obtained. In the event of the limb not being properly balanced when the hooks are placed at the ends of the bar, the hook on the depressed end should be moved to the notch nearer the centre of the bar, as shown in the sketch. The equilibrium will be thus obtained, as in a steel-yard.

The mode of connecting the limb with the suspender may be left pretty much to the choice of the surgeon. If the fracture be treated with the ordinary side-splints, or the starched bandage, then the limb had better be laid on a slightly curved back-piece, (supplied with the suspender,) and slung with straps from the hooks before-mentiond. Where the limb is treated with a back-splint, as M'Intyre's, or lies in a fracture apparatus having a back-piece, the connexion can be made by the aid of straps without the addition of another back-piece.

I was once asked by a medical man whether I could devise a method by which a patient with a fractured leg could turn over on his side? A very slight addition to this apparatus would enable such patient to accomplish that feat; but as the act of turning would necessitate muscular exertion, and thus displace the fragments, I have not thought it necessary or prudent to carry out the idea.

In the application of this apparatus, the base should be placed *underneath* the mattress in the required position for the limb, and tied on its outer side to the frame or battens of the bed. If possible, this should be done before the patient is placed on the bed. The fracture having been attended to, the remaining portions of the apparatus are placed in position by dropping the pillars on the stumps which appear at the edge of the mattress. Connexion can then be made between the limb and the suspender, and any inclination of the top from the horizontal position can be rectified in the manner before stated.

At what height the limb should be raised above the surface of the bed, is a question on which difference of opinion exists. As a rule, the patient is most at ease when the limb is only slung sufficiently high to clear the bedclothes underneath.

The advantages embodied in this instrument are (in addition to the longitudinal and swinging movements): a transverse motion, the most frequently required; a rotatory motion, which could only be imperfectly obtained in an ordinary two-sided "cradle"; and, the form of the instrument being unilateral, the sound limb is not thrown aside from its fellow by any intervening framework, thus preventing distress in the hip-joint. This peculiarity of form also enables the surgeon or the nurse to dress any wounds, supposing the fracture to be compound, without removing the suspender from the bed.

Many modifications of this instrument might be suggested. I have one now in use, with a single telescope pillar reaching to the floor, and fastened to the frame of the bed with a screw-clamp; but I consider the present form most to be depended on for stability.

The chief difficulties I had to contend with in the construction were, to gain free longitudinal movement when the line of suspension was out of the centre—that is to say, when the lower trucks were at one end of the transverse rail (as seen in the sketch); also, as to the mode by which attachment to the bedstead should be made. These have both been overcome. If, however, by any accident, the upper trucks drag when the lower trucks are in the position just named, they will right themselves by the patient merely bringing his leg underneath the centre of the transverse rail before he raises or lowers himself in the bed; but if the travelling-gear be made and put together properly, no dragging will take place *whilst the leg or any weight is suspended*, as the upper trucks are thereby prevented rising off their rail. A little oil should be occasionally smeared over the parts subject to friction.

I have instructed the Messrs. Weiss on certain points of detail, apparently unimportant, but on the carrying out of which the proper working of the instrument depends.

I have also desired them to make a simple and inexpensive unilateral framework, without travelling-gear, for relieving any part of the body from the pressure of bedclothes. I have termed this a *bed-guard*. The apparatus just described can be used for this purpose by merely removing the travelling-gear.—*Lancet, Feb.* 24, 1866, *p.* 202.

52.—LEATHER-FELT SPLINTS.

These very simple and admirable splints are the invention of Mr. J. G. Hides, of Mortimer-street. They consist of stout felt, lined on one side with wash leather. All that is necessary for their application is to place a splint (with the leather side downwards) upon a table, then paint the felt side with a liquid which is supplied with them, apply the splints to the injured limb, and bandage with an ordinary roller, leaving a little interval between each turn of the bandage. If properly used, the splint will harden almost like a piece of wood in about thirty-five minutes, taking the temperature of the limb at 100° F. The advantages over other appliances seem to be their lightness, perfect adaptability to parts, and, as no padding whatever is required, they are more quickly and easily applied, and the bandage uniting with the splint as the liquid dries more support is given.

They can be used again and again by merely painting the felt side afresh with a weaker solution of the liquid. They are cheap, the price for a set of eight arm and hand splints being about the same as that of wooden splints. If additional support be required, the felt has only to be made of thicker quality.

We believe that they have been much approved of by Mr. Partridge, of King's College, and Mr. De Morgan, of Middlesex Hospitals. We have ourselves tried them for a fractured arm, and had every reason to be satisfied with them. The fracture was rapidly put up, and had the appearance of lightness and neatness so delightful to a Surgeon's eye. We fancy they would be capital splints for field practice in the army.—*Med. Times and Gazette, Feb.* 10, 1866, *p.* 162.

53.—HOW TO MAKE GUTTA-PERCHA SPLINTS.

By Edwin Truman, Esq.

No material presents so many advantages for surgical purposes, with so few drawbacks, as gutta-percha; and a very little experience will enable all surgeons to use it perfectly. In a periodical of this year this passage occurs: "After a good deal of hard work I succeeded in thinning down the substance to the required thickness." The writer is describing the production of a gutta-percha splint for putting up a fractured jaw. It seems strange to hear so kindly a substance so spoken of—dough is not more easily moulded and flattened into sheets in the hand of the cook than gutta-percha might be—and by the same means a paste-board and rolling-pin which would have despatched this "hard work" in a few seconds, and produced a sheet of any thickness or size, perfectly even and ready for the splint. No matter in what form the gutta-percha is, nor in how many small threads or large balls,—place it in a vessel of water, and boil it till it is perfectly softened throughout; now collect it into one ball; dip it in cold water to enable it to be handled without burning, and knead it into a uniform mass with the hands. To do this, if it is very good and tough, will require some strength and also warming two or three times: but eventually it will form a uniform plastic mass not much harder than stiff putty. When in this state, place it on my flat surface which is wet, and with a roller—a common glass rolling-pin dipped in water is the best—roll it into a sheet of the desired thickness, let the roller slide upon it in all directions, and continue this action till the surface is uniform; it will now be fit for cutting into shape. This can readily be done either when hot or cold with a pair of scissors, and no force is required to mould it into the form of splint required, the only precaution requisite being to hold it steadily in that shape till it is cold; after which it will not alter unless again made hot. It may sometimes be requisite to join two pieces of gutta-percha together. This is readily done by warming the two surfaces to be united by dry heat, with a hot iron, not burning, but merely softening the surface. Two such hot, dry, soft, clean surfaces will unite most perfectly when brought into contact with each other, if held together till they are cold.

It may sometimes also be useful to know how two pieces of this material may be moulded on to each other without sticking. If one be cold and the other hot, they will not unite, especially if the cold surface be anointed with soap-suds.

It is, I believe, the custom in some of our London hospitals to look upon an old gutta-percha splint or one which has aborted, so to say, in the making, as useless and only fit to be sold as old material, thus necessitating a considerable loss. If the above described process were employed no small saving might be effected, as the same piece of gutta-percha might be used over and over again.—*Archives of Dentistry.—Edinburgh Med. Journal, May* 1866, *p.* 1036.

54.—ON A CASE OF RECURRING FIBROID DISEASE OF THE SHOULDER.

Under the care of Sir WILLIAM FERGUSSON, at King's College Hospital.

[Sir William drew attention to the appearance of the hand and arm which had just been removed from a patient on account of recurring fibroid disease of the shoulder. Two-thirds of the scapula had been previously excised, and, in the present operation, the remainder of that bone along with the greater part of the clavicle.]

The disease being confined to its points of junction with the trunk, the limb itself, when the affected portion was covered with a cloth, showed no sign of the cause which had necessitated so severe a procedure. Round, white, and shaped with more than ordinary beauty of proportion, it reminded one of a marble model in a sculptor's studio. Pathologically, this unimpaired preservation constituted a point of scientific interest. It suggested at once a line of distinction between the disease with which the patient was affected and the malady with which such an affection was formerly classed—cancer. There was here none of that wasting which could scarcely have been absent in a disease of the latter class existent—as this has been—for five years. And this is a point which has been noted by Mr. Paget, who first proposed the name "Recurring Fibroid" as distinctive of this disease. He says (art. Tumours, "Holmes's Surgery"): "Moreover, after repeated recurrences, the patient commonly retains apparently good health, and shows none of that cachexia which would almost certainly exist in a patient who had suffered repeated recurrences of cancer." A short reference to the observed characteristics of recurring fibroid disease, as graphically described by Mr. Paget, will be useful to illustrate the reasons for Sir W. Fergusson's operation.

The great feature of this disease is obviously its liability to recurrence. But this character belongs also to cancerous disease. Recurring fibroid, however does not affect the lymphatics, and its recurrence is in the situation which it formerly occupied. The system generally does not appear to be affected by the disease. In the few cases where, very late in the disorder, similar growths have occurred in the lungs, it may be suspected that the propagation was by direct transport of germs in the blood from vessels into which a tumour had grown. "The general appearance of recurrent tumours is, in their first growth, often not distinguishable from that of tumours of the same structure that are not likely to recur; they may look like ordinary fibro-cellular, fibrous, or other tumours. More often, however, they differ from these ordinary tumours in that they are softer, more succulent or glutinous, glistening or translucent, brittle, or easily torn, with a kind of fibrous grain. Commonly, all these peculiarities become more marked in the successive recurrences of the tumour; and the latter growths are, generally, like masses of yellow or ruddy soft gelatine with bloodvessels. The latter are, also, usually much more rapid in their progress than the earlier growths, and are, generally, less well-defined, penetrating further and more vaguely amongst the interstices of adjacent parts."

There seems scarcely a limit to the number of times that these these tumours may recur.

[There was a patient in St. George's Hospital a twelvemonth ago, in whom recurring fibroid disease had been removed forty times, and yet recurred, yet the removal should always be effected if possible, for the patient may regain perfect health and the disease cease to recur. There have been a fair number of cases recorded in which part of the clavicle and the scapula have been removed successfully. In January 1865, Sir W. Fergusson removed the lower two-thirds of the scapula in a young woman on account of this disease. The wound healed and left the arm freely moveable. Since then a tumour appeared under the pectoral muscle, and increased so rapidly that it was determined to operate at once.]

The patient having been placed under chloroform, a grooved needle was thrust into the upper part of the tumour a little below the clavicle, at a point where it seemed just possible, from an obscure sense of fluctuation, that fluid was present. There was, however, none. A small incision was then made over and along the clavicle about an inch and a half external to the sterno-clavicular joint, through which the bone was divided by the saw and cutting pliers. The object of this, as Mr. Fergusson afterwards explained, was to allow free movement of the shoulder during the ensuing steps of the operation, without causing any strain upon the sterno-clavicular joint. By this step, too, implying the preservation of the inner end of the clavicle, the sterno-mastoid muscle was reserved entire. An assistant (Mr. Wood) then thrust his thumb through this wound and compressed the subclavian artery upon the first rib. Next, the incision was continued along the clavicle, at first outwards, then backwards over the acromion, and lastly downwards and forwards, so as to terminate in the inner and upper part of the arm below the axilla. From the point where this incision, leaving the clavicle, tended backwards, another was made passing down in front of the shoulder-joint, and meeting the first at an acute angle. By these means two semilunar flaps were formed, one before and the other behind, and the skin of the axilla was preserved. The tumour having been exposed by dissecting the flaps from its surface, the muscular structures which attached it to the trunk were divided. There still remained to be accomplished the section of the subclavian vessels and the accompanying nerves, and this was the most delicate part of the operation. Behind the clavicle the tumour was less distinct than at any other parts, spreading vaguely amongst the tissues, and rendering it doubtful at first how far it might extend amongst the muscles of the neck. A careful dissection succeeded in completely isolating it. The mass was then drawn forwards, and the subclavian artery was compressed. In order to obviate the chance of slipping, a strong forceps, such as is used for removing sequestra, had been prepared by having its teeth covered with wash-leather. The blades of this were pushed from behind forwards so as to enclose the subclavian vessels, and another instrument of the like kind was pushed from before backwards with a similar object. Thanks to these, which admirably answered their purpose, there was no difficulty in retaining and ligaturing the artery, and the operation was completed by finally dividing the remaining tissues, chiefly nerves and vessels, outside of these blades, with the loss of scarcely a tablespoonful of blood. For precaution's sake, ligatures were applied to two or three other vessels, but they were scarcely needed. The flaps were then brought together, sutures applied, and the patient removed.—*Lancet, Nov.* 25, 1865, *p.* 592.

ORGANS OF CIRCULATION.

55.—ON THE GALVANO-PUNCTURE OF ANEURISM.

By Dr. John Duncan, M.A.

[The patient in this case was 45 years of age and complained chiefly of shooting pains in the shoulders and upper part of the chest. In December, 1863, a small pulsating tumour appeared through the sternum.]

The aneurism now, in October, 1864, occupied nearly the upper half of the sternum, stretching beyond it above and to the sides; in shape, a flattened hemisphere, quite without nodules, and with a moderate pulsation. From that time it progressed slowly for a year, extending more upwards than downwards, sometimes stationary, never retrograding. Notwithstanding its increase, Mr. A.'s general health remained so good that he was able to follow uninterruptedly his usual vocation, concealing the unsightly deformity by comforters and a loose great coat. He was, however, greatly annoyed by flatulent dyspepsia, and grew thinner and paler and weaker. He had little of his former pain, but his nights became restless, his cough troublesome, his breathing difficult when he lay upon his back, and the necessary use of opiates increased the stomachic disorder. In October 1865, the acetate of lead was tried and pushed to plumbism; for about that time an apparently successful case had been recorded by Dr. Owen Rees. It had absolutely no effect. The tumour grew faster than ever, and by the 10th of November had acquired a bulk truly enormous. So high did it reach in the neck, that as he sat, stooping from weakness, his chin rested upon it; and he asked me one day if it might not push back his head by its increase. It covered half of the sternum and of each clavicle, and from side to side over its highest part it measured thirteen inches. It was no longer smoothly hemispherical. Nodulated eminences had arisen here and there. Now one and now another had rapidly increased, reddening the skin, and threatening to burst, but hitherto they had failed to do so. At length, however, there came at the upper part a small vesicle, the size of a pea, perhaps from friction of the coverings. It was smeared with lard, and the softest lamb's-wool was placed between the clothing and the skin. Thus treated, it shrivelled and disappeared in a day or two; but, on the 15th of November, another of the same size filled with bloody serum arose lower down, where the skin was extremely thin. As this tightened and puckered with the pulsation, it seemed as if every moment it would give vent to a gush of blood. It burst in a day or two, but exposed simply the raw surface of the skin as after a blister. This surface then took on a slow ulcerative action, deepening little but gaining in superficies, while one or two new vesicles formed around it, which either dried up again or added to its extent. Matters progressed thus, while the patient's strength rapidly failed, till, on the 2nd of December, a very slight oozing of blood took place, which was stopped by applying the perchloride of iron solution. It was evident, however, that, if nothing were done, the patient could live but a day or two at furthest, for bleeding from some point of the extensive ulceration must speedily recur. What was to be done had of course been anxiously and often thought over and discussed.

My father had already made an essay with galvanism for the cure of an abdominal aneurism which was threatening to point in the posterior lumbar region. Fifteen minutes after the introduction of the needles, pulsation had entirely ceased, and the operation had been performed without the slightest pain or inconvenience to the patient. Unfortunately this person afterwards resumed his occupation as a post-boy. In a fortnight he rode to Roslin, and indulged in a quite unwarrantable amount of exercise. That night he died suddenly, it was supposed by the medical man who saw him from rupture of the aneurism internally. To the day of his death he felt perfectly well.

The case was encouraging, and it was determined as a last resource to apply galvano-puncture in the present instance. The injection of perchloride of iron was evidently too dangerous, for as each drop formed its coagulum it might easily be carried into the circulation; and the fatal result attending the only attempt to introduce wire into the sac of such an aneurism gave little encouragement for a repetition. On the other hand, galvano-puncture had been attended with a large amount of success, and the needles gave a holding point for the clot during its formation.

It was thought good, therefore, to request the assistance of Dr. Wright, whose knowledge of matters relating to natural science is well known, and who had been conjoined with my father in his former experiment. Everything had been prepared for the evening of 3rd December, when, unfortunately, at four o'clock that day, I was hurriedly summoned to the patient on account of copious hemorrhage. It had already greatly ceased under the perchloride of iron, and a little further pressure arrested it entirely, but not before the patient had been rendered very faint and the pulse very feeble. I at once sent for my father and Dr. Wright, and also for Professor Simpson, who was desirous of witnessing the operation.

By six o'clock everything was ready, and the patient was placed on his back upon the sofa. Two needles were introduced, one on each side of the sac not far from its base, and were so placed that they must have overlapped each other by an inch at the point, while lying about that distance apart. The conducting wires were attached after the needles had been introduced, and a very feeble sensation accompanied the closure of the circle. The patient thought that he experienced a slight shock also on its being opened, but nothing was felt during the whole period of galvanic action. Three-quarters of an hour after the current began to pass, the intensity of the pulsations had decidedly diminished, and the wires were transposed at the battery, so that the positive needle now became negative. In an hour and three-quarters the pulsation had still farther diminished, but without any measurable change of bulk in the tumour. By percussion, air could be mapped out on the surface in little wells, principally to the right front. As it increased in quantity pretty rapidly, and its entrance into the circulation was feared, the needles were removed at five minutes past eight o'clock, or two hours and ten minutes after their insertion. Towards the end the pulse had become a little more feeble from the long-continued restraint in one position, which Mr. A. bore with difficulty in his exhausted condition, though fortunately he had not experienced that impediment to respiration which was usual when he lay upon his back. Some brandy was administered; he was undressed and put to bed. He was directed not to turn too much upon his face, that all danger from the gas might be avoided.

4th December. He had passed on the whole a comfortable night, sleeping well towards morning. The pulse was 80, rather feeble. The force of pulsation in the tumour, though greater than last night, was decidedly less than before the operation. At eleven o'clock, a very faint tympanitic note was yet elicited by percussion on certain parts of the tumour, but it disappeared in the afternoon.

At half-past seven in the evening, two needles were again introduced a little higher than before. They were more than twice as thick as the first pair, and somewhat longer; so that they must have overlapped nearly two inches. Their action was much more rapid than on the former occasion. In fifteen minutes there was clear percussion at parts, and the poles were changed. In five-and-twenty minutes the tympanitis was very extensive, and it was thought right to remove the needles. The air, as before, was principally on the right side, but much more was formed in this than in the first operation, and some even escaped along the needles. A very notable diminution in the force of the beats was observable.

5th. He was restless during the night. He would often start from a doze, in the belief that blood was trickling down his side, and that he was dying of hemorrhage. In consequence, he was feverish a little, and exhausted in the morning. The tumour in parts gave the sensation of dough, in others, of a half-empty bladder. A few drops of bloody serum escaped by the puncture of the second pair of needles.

6th. He was extremely well, having passed the night in quiet sleep. His pulse was about 80, stronger than it had been, and he enjoyed taking small quantities of arrow-root, calf's-foot jelly, and beef-tea. In the tumour very little air could be detected, and the pulsation remained moderate.

On the 7th, he suffered a good deal from flatulence, and could take almost no food. A vesicle, dark and containing some air, had arisen during the night. It was higher up than the ulcerated surface, which was still covered by the lint steeped in perchloride of iron. In the evening his morphia draught was increased from 60 to 80 minims.

8th. He had slept well and felt much refreshed. An ulcer now occupied the site of the vesicle, and, as the discharge was a little fœtid, Condy's fluid was applied. He continued lively throughout the day; but next morning was very much worse, and he rapidly grew weaker, suffering greatly from flatulence and colic, till he died on the morning of the 11th December.

Electro-puncture was revived by Petrequin in 1845, and a considerable amount of experience has since been accumulated. Many experiments have been made on the occlusion of arteries in animals, chiefly by M. Abeille, and by commissions of the Milan Scientific Society. Their results were thoroughly satisfactory.

Thus, in eleven experiments on dogs and sheep, M. Abeille invariably found, on killing the animal a few days afterwards, a firm and even laminated clot closely adherent to the vessels. M. Asson reports, as the result of the labours of an Italian committee, that galvanism is a mode of producing a firm obstructing clot, which is composed of fibrinous granulations, connected together and adherent to the walls of the vessel. It has been asserted that this is due to inflammation excited by the needle or galvanic current in the walls of the artery; but I have found that, if the animal be killed immediately after making these experiments, a clot already exists at the point operated on; and Phillips observed that it required forty-eight hours to obstruct a vessel by needles through it.

About sixty cases of aneurism or other tumours containing blood have been treated by this method in the human subject.

[In a table given in Ciniselli's Memoir on the subject, out of 50 cases treated by electro-puncture twenty-three were cured, twenty were not cured, and seven died.]

The most common and important accident which follows electropuncture is inflammation of the sac or of the surrounding tissues. More than half of the total number of patients thus treated have suffered from it in a greater or less degree. In the sac it may be produced by the simple rapid deposition of a clot, whatever be the means employed; but much more often in the cases before us it was excited by the chemical action of the electric current. This, however, is a form of the accident, which, although it may be followed by unpleasant or even fatal results, yet not unfrequently, when slight, tends to a prosperous issue by its own proper action. In fact, a very considerable number of the cures have been consequent to it. A much more serious effect of the current is cauterization of the skin or of the aneurismal wall in the course of the needles. This has occurred only too often, and has led, as might be expected, to troublesome complications in the shape of abscess, sloughing, and hemorrhage. In some a slough has been at once produced, and bleeding has accompanied its separation, or has been restrained only by the coagulum deposited at the same time, or by the use of styptics over the ligature. In others an abscess has formed in the wall after a varying interval, and has opened externally, in many, fortunately, with no bad result.

The most intense pain is often experienced during the operation, so great, indeed, as to cause convulsions and alarming syncope. A patient of M. Petrequin utterly refused to allow a second introduction of the needles, though success seemed certain. M. Abeille, in describing his very successful subclavian case, states that the patient, having emerged from the influence of sulphuric ether during the operation, uttered the most piercing shrieks, and was fearfully convulsed. "She poured," he says, "with sweat. No surgical operation can cause such tortures."

This pain, however, is found to be present only when the other accidents I have mentioned are so; and many have experienced nothing further than the shock upon opening and closing the current which was felt by Mr. A. This shock, of course, must generally exist; for, although the blood itself is not sensitive, it conveys the electricity to the wall, just as, if the hand be immersed in water when an electric current is acting, the influence of the wires will be felt even though they be not touched. Such a sensation, however, is hardly perceptible except with a powerful battery. Pain would, indeed, be felt were the interrupted current used, as it not unfrequently was; but for other reasons that form of galvanism ought not to be employed.

Leaving out of account the accidental puncture of a nerve, pain then depends on cauterization in the aneurismal wall.

This, as also the main source of danger, we must aim at avoiding, and I think we are in a position to do so. In most of the operations the needles were coated with varnish, having a certain length at the point uncovered. But this was found to be unsatisfactory. The varnish was imperfect or stripped in introduction, and cauterization still often took place. Restelli complains that in conducting his experiments he could get no varnish to effect perfect insulation. Ciniselli, impressed with the fact that this was almost the sole source of danger, and believing that no insulation could be made perfect, supposed that he could prevent the action on the tissues where it is most serious, viz., at the negative pole, by passing through it a current of positive electricity in the first place. The object may be attained, as was proved in Mr. A.'s case, by coating the needles with glass; although it also has the disadvantage of being liable to strip. One of them did so, and it was withdrawn, covered with sealing-wax, and re-introduced by its original puncture. Dr. Wright had suggested the use of vulcanite, which could be polished to perfect smoothness with the rest of the needle, but, through a mistake of the maker, glass was applied to the second as well as the first set. There is, moreover, a little difficulty in applying vulcanite. It is evident, however, that glass, sealing-wax, caoutchouc, or gutta-percha, will act as perfect insulators, and can be readily enough applied to the metal. I have found them to be so in various experiments, and prefer, on the whole, a mixture of shellac and gutta-percha. If care be also taken to avoid touching with the unprotected point any portion of the inner surface of the sac, there is no danger from chemical decomposition; while thermic action is easily prevented by merely keeping the electrodes sufficiently apart.

Another source of danger may be avoided by a very simple precaution. The needles should be of the smallest size consistent with sufficient strength, for the tense wall of an aneurism is liable to inflammation from slight causes. In Mr. A.'s case the first pair were fine, and their course was invisible at the post-mortem examination. With a view to more rapid action the second pair were made as thick as stocking wires, and the consequence was the escape of air along their sides, and slightly into the cellular tissue, extravasation of blood in their course, and on the left side the formation of a drop or two of pus. The electric action may be increased, if required, by increasing the number of electrodes, which might be united together after insertion. Several minute punctures are evidently better than two large ones. Such a course would, moreover, afford a large number of points for subsequent fibrinous deposit, if the sac were not filled with coagulum at one sitting. If, again, it be true that slowness of formation adds to the firmness of the clot, we may thus have it so without retarding the operation as a whole. Not that we should follow the example of the operator, who inserted no less than fifty needles into a popliteal aneurism, and who had, on withdrawing them, to combat hemorrhage, not a little alarming.

The dangers of introducing air into the circulation, and of producing emboli, have been much discussed. No inconvenience has been experienced from either in the past, and but little apprehension need be entertained for the future. The tough gas-containing froth tends to rise to the surface, is very little likely to be carried into the circulation, and is so rapidly absorbed that it can never pass the capillaries. Moreover, to produce untoward effects, the introduction into the heart of a quantity of gas much larger than can thus be produced would be required. The clot, again, adheres so closely to the needles, and is in itself so consistent, that with difficulty could any portion be carried away. Many experiments have been made on animals with arteries unligatured and uncompressed, and in none, so far as was observed, did gas or emboli pass down the vessels. In the human subject the artery has often been obliterated along with the aneurism, and even then no embolism has ever taken place, though the arteries were leading trunks of the body.

We have thus glanced at the dangers. Let us examine now the modes in which cures occur. They are three in number.

1. In a few cases the surgeon, on concluding the operation, fears that it has failed; but, after some hours or days, observes the tumour gradually become harder, till at length its pulsation disappears, and in time it is slowly absorbed.

2. Sometimes, on the other hand, the pulsations cease, and the aneurism becomes firm during the action of the current. One of two results may then follow. With the majority condensation and absorption proceed; but pulsation may, I believe, return, through partial melting of the clot, and the aneurism remain half cured, or be filled up by subsequent deposit of fibrin.

3. A third mode is by inflammatory action, which begins to show itself in some almost immediately, in others after the lapse of a day or two, by the usual local and general symptoms. These subside (but rarely without giving ground for alarm) and the tumour is left fully solidified.

In order of frequency, cures have taken place most often with inflammatory accompaniment; next, by immediate and complete coagulation; lastly, by partial clot, with subsequent fibrinous deposit. But the most frequent is precisely that which we wish to avoid; and our efforts must be directed to attain such thorough mastery of our instrument as may enable us to produce invariably one of the two last mentioned. We approach this certainty by ascertaining the causes of accident; but a knowledge of the blood-changes induced by electric currents would greatly assist our endeavours. Various theories have been framed to account for the coagulation, but none of them are entirely satisfactory. According to some it is a special action of electricity unknown and *sui generis;* while others hold that an elevated temperature in the traject of the current is the efficient cause. But the most popular view held by Bellingham, Restelli, Apjohn, and others is, that the acid produced at the positive pole by decomposition of salts in the blood is the agent in coagulating the albumen. Althaus and others even go so far as to recommend that the negative pole be not inserted at all, but applied to the aneurismal wall as a flat plate. This is not only unsatisfactory, because, with a sufficient battery the skin might be cauterized, and the decomposing power lessened, but is founded on a theory, to a certain extent, erroneous.

To determine the point we are discussing many experiments have been made on all kinds of animal fluids, and with the most contradictory results. Passing these over let me refer to some observations made by Smee and Van Deen, which are especially valuable, because they were made without the slightest reference to the medical application of electricity. Dr. A. Smee has shown that by passing oxygen through an albuminous fluid, a substance resembling fibrin may be obtained, and that this transformation is assisted by a temperature of 98° to 110°, by making the fluid slightly acid, and by the presence of a serous membrane in it. He has also shown that by the passage of hydrogen through albumen, jelly-like and viscid substances may be produced, which he calls artificial chondrin and mucin, and that alkalinity assists their formation. These actions are extremely slow, are rapid only when the oxygen and hydrogen are produced by the electric current from the fluid itself, and then probably there are other agents at work. It is interesting to note that Van Deen, in a series of experiments on the chemical action of electricity, obtained from albumen, among other things, a substance which he characterized by putting the word fibrin within brackets, and with a mark of interrogation after it.

With the help of my friend Dr. Fraser, to whose skill in these matters I am largely indebted, I commenced some experiments on albuminous fluids. They are by no means sufficiently numerous or varied, and we intend to continue them; but some of the results we have already obtained may not be uninteresting. We used a Bunsen's battery of six cells, with generally platinum wire at the extremity of the conductors, and contrary to the assertions of Baumgarten, and more especially of Steinlein, the platinum proved a most admirable pole.

From experiments on white of egg we obtained the following facts :—

1. With the continuous current the characteristic action was produced rapidly, even with the poles four inches apart ; but it increased as the electrodes were approximated, till, with one inch between them, they so coagulated in half an hour nearly one cubic inch of the fluid that nothing was spilled on the inversion of the glass. Parallelism or obliquity of the poles made no perceptible difference.

2. The interrupted current was almost inert till the electrodes were brought within a quarter of an inch of each other, and even then the coagulation was very slow, though of the same character as the other. As to the nature of the coagulum, our results correspond very much to those obtained by Smee, varying somewhat according to the specific gravity and nature of the fluid. We have acted on pure albumen of high and low density, on impure albuminous fluids, such as serum, defibrinated blood, the contents of a hydrocele, as well as upon blood itself in and out of the body. In every instance have we obtained a coagulum, and everything tends to the conclusion that in an aneurism the froth and jelly which arises at the negative electrode, would last, at all events, sufficiently long to form a most efficient nucleus, while the opaque mass at the positive pole would permanently remain unaffected, either by the water or any amount of alkalinity in the blood. It would make this paper too long to detail our numerous experiments, in the course of which we met with several interesting facts, which we propose to arrange in another essay. Let me only say, that in the blood itself we have observed that three substances are produced, viz., a viscid froth by the negative electrode, a black coagulum very distinct in the midst of florid arterial blood at the positive, and in addition to these a quantity of dark defibrinated fluid. In the living arteries, also, we have succeeded in producing coagula with each pole separately, as well as with the two in the same vessel. It is difficult to estimate the value of the treatment by galvanism. Judging simply by its past performances, many have thought that it should be restricted to aneurisms beyond the reach of other means, or where such means are so dangerous as to be practically unavailing. I have shown, however, that most of the accidents have arisen from faulty apparatus and manipulations ; and I believe that if the following precautions were taken our statistics would be extremely favourable :—

1. The needles should be carefully insulated.

2. They should be prevented from touching each other or the sac, and to attain this are best introduced from the same

side, parallel to each other, and from one to two inches apart. Thus, also, their action, while sufficiently rapid, is not too localized.

3. They should be extremely fine, but may be multiplied according to the size of the aneurism. They should be made of steel, but had better be coated with gold, as the steel is eaten away with great rapidity, and the surface acting is thus diminished.

4. A battery of medium strength, and with a continuous current, should be employed. Four to eight of Grove's or Bunsen's cells are sufficient for a large aneurism.

5. The action may be continued till pulsation stops, or gas can be clearly detected by percussion.

Our knowledge of the immediate action of electricity on the blood is not yet so thorough that we can confidently predict success, but we can, I think, so eliminate sources of danger, that only want of success will be the result in any case. Electro-puncture might, therefore, be employed, and for myself I would not to hesitate to employ it wherever the arterial orifice is of moderate size, and I believe it might even be farther extended. Cirsoid aneurisms, and those not seated on the main artery of a limb, are, of course, especially well adapted for its use ; and in cases similar to Mr. A.'s it affords the only, but that a good hope of relief.

It is deserving of much more attention than has been accorded it in this country, were it only on account of the admirable cures effected by Petrequin, Abeille, Ciniselli, and Nélaton.—*Edinburgh Medical Journal, April* 1866, *p.* 920.

56.—ON LIGATURE OF THE FEMORAL ARTERY.

By Professor SYME, Edinburgh.

[In a paper in the Edinburgh Medical Journal for 1841, Mr. Syme showed that ligature of the femoral artery could be applied almost with perfect safety. His words were]

"It is established that the great sources of danger from the ligature of large arteries are undue laceration and separation of the connexions of the vessel, whence hemorrhage is apt to ensue, and injury to the coats of the veins, which is apt to occasion inflammation and an obstructing coagulation, causing mortification of the limb. The subclavian artery, when tied at the external edge of the scalenus, lies at some distance from the vein, and neither the carotid nor the external iliac artery adheres so intimately to its accompanying venous trunk as to render it at all difficult or dangerous to pass the needle. But the femoral artery has a closer connexion with the vein, and though it is felt by the operator's finger, after the fascia has been opened, round and distinct, and as if insulated from the surrounding parts except by the loosest connexions, any attempt to pass the ligature without further dissection, either proves abortive, or, if executed by force, exposes the patient to the greatest danger. I have seen a gush of dark-coloured blood proclaim transfixion of the vein. I have seen, on dissection, a portion of this vessel included in the ligature; and I have also seen the external coat alone grazed as it were by the needle, but, nevertheless, excited to fatal inflammation. If, on the other hand, this danger be avoided by using blunt instruments, or the finger, to detach the artery from its connexions, the patient is exposed to the hardly less disastrous consequences of hemorrhage through ulceration or sloughing of the vessel.

"To tie the femoral artery safely, the surgeon should be impressed with the conviction that the operation is one not of difficulty but of great nicety. He should make an incision between two and a-half or three inches long in the proper situation, cut through the fascia to a smaller extent, and expose the sheath of the vessels. So far he can hardly go wrong, but then, instead of hastening to pass the needle, he should by ligature, or the temporary application of spring forceps, close every little vessel that discharges enough of blood to obscure distinct vision of the object he has in view. Let him now seize the sheath with dissecting forceps, and, gently raising it, make a small opening by means of a straight narrow sharp-pointed knife. The cellular and fatty substances which envelope the vessels in variable quantity are next to be elevated and divided in successive portions until the external coat of the artery appears quite distinct and *white*, when the needle may be passed without the slightest difficulty or danger."

At the time when this was written, my experience did not extend beyond seven cases, all of which had proved successful; but since then there have been twenty-eight more, so as to make in all thirty-five, and in none of these has the operation proved unfortunate with the exception of one, which terminated fatally from suppuration of the sac. But even on this solitary occasion the result could hardly be attributed to the operation, as the parts concerned with it were found perfectly sound and free from the morbid condition, which seemed distinctly referable to the derangement of health that had been induced by a prolonged attempt to cause coagulation through pressure at the groin. This treatment, continued for many days and nights had disordered the patient's sleep, appetite, and mental composure, so as in all probability to excite a proneness to unhealthy action. Should such an explanation not be deemed satisfactory, the result of my experience in ligature of the femoral artery would still be one death in thirty-five cases.—*Edinburgh Med. Journal, May* 1866, *p.* 967.

57.—ON THE TREATMENT OF POPLITEAL ANEURISM.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

S. W., aged 44, married, a tall and healthy coachman. Six weeks ago he "strained" his left leg while carrying pails of water, and immediately afterwards felt stiffness of the knee, with severe cramp down the calf and in the sole of the foot. On examining the ham he found a tumour larger than a duck's egg. Within a few hours of the accident the leg below the knee became swollen and œdematous, but the swelling subsided in a few days. He has continued at work till now.

Dec. 18th, 1865.—*State on admission.*—There is a spherical tumour, with a diameter of two or two and a half inches, situate above the centre of the popliteal space, soft and easily compressible, and pulsating regularly. On flexing the tibia to an angle of eighty degrees pulsation is arrested in the anterior and posterior tibial arteries; and on flexing still further, the tumour itself ceases to pulsate. Pulsation is also readily stopped on pressing the femoral artery.

Treatment and progress of case.—Ordered to keep his bed. Leg bandaged and elevated, so as to empty the superficial veins, which are large, varicose, and full, especially about the calf. One grain of opium at bedtime, if restless.

Dec. 26th. Leg firmly bandaged, and knee bent to an acute angle so as entirely to stop pulsation in the aneurism.

30th. On examining the aneurismal tumour, it is found to be only half its former size and much harder, yet still pulsating. Pulsation in the superficial femoral artery scarcely distinct; an inch below Poupart's ligament the circulation is carried on mainly through the profunda. Leg rebandaged, and again bent at the knee-joint; a double tourniquet, which stops all pulsation in the tumour, is applied over the superficial femoral artery, and a pad is strapped over the aneurism itself.

Jan. 5th, 1866, (ten days after first flexion; six days after applying tourniquet). Tumour about the size of a walnut, hard and solid, not pulsating. It has continued the same till the present date, Jan. 16th.

The man at first had severe cramping pains in the calf and the sole of the foot. Some sudden dilatation of the popliteal artery would perhaps explain these pains, pressing, as it probably would, upon the internal popliteal nerve, and causing the pain and cramp referred to. The popliteal vein, lying between the artery and the nerve, would be pressed upon by the aneurism; and the pressure interfering with the return of blood by the vein would cause the œdema of the leg, which is reported amongst the earliest symptoms of the injury.

This aneurism was cured by simple flexion, for which method of treatment the case seemed to me to be peculiarly adapted. The aneurism was situated near the *upper part* of the popliteal artery, close to where it passed through the adductor magnus, and consequently in a comparatively fixed spot. I take it that the middle of the popliteal space is that spot which is opposite the interval between the tibia and femur. Now, the aneurism was altogether above such a centre—to which fact, I think, may be in part traced the readiness with which it was cured by flexion. Bearing in mind what I have already told you, that the artery where it passes through the adductor magnus is comparatively fixed, I think you cannot fail to perceive that, when the leg is flexed to an acute angle on the thigh, the altered direction of the continuous trunk of the popliteal artery is so sharp, so bent upon itself, that it tends to stop the supply of blood to all the parts below it; whereas, when the aneurismal enlargement is placed lower down, the leg cannot be so completely fixed, nor the artery so changed in its course as to interfere with the transmission of blood. I have seen several cases of aneurism situated in the middle and lower portions of the popliteal space, which could not be cured by long-continued flexion. The explanation just given is probably sufficient to account for the want of success. My belief is, that the higher the aneurism is situated in the popliteal space, the greater is the probability of the cure being effected by flexion, provided only that there be a short piece of the popliteal above the aneurism.

One element against the favourableness of this case was the fact of the tumour being very soft—its contents quite fluid. You know that an aneurism is cured by layers of fibrin forming on the wall within the sac. When these layers have been deposited, they communicate a feeling of hardness to the touch. The absence of this special sensation, therefore, indicated that no layers of fibrin had been formed; and that, consequently, nature had not yet commenced to cure the malady.

The principle of the treatment was to arrest ultimately the flow of blood through the popliteal vessel. Now, it was found that by bending the knee to an angle somewhat less than that of eighty degrees all pulsation was stopped in the anterior and posterior tibial vessels, and in the tumour itself; so, if the man could bear that position for a few days, it was almost sure to cure his aneurism. No alteration was made in his diet; nor was any important medicine given to him. He was simply compelled to keep in bed; his leg was bandaged and elevated on a pillow, so as to empty the varicose veins and get rid of any œdema there might be below the knee. And after eight days, the leg being firmly bandaged to prevent any swelling, the circulation was entirely arrested in the sac by the knee being fixed at an acute angle. You will remark the simplicity of the means adopted to accomplish all that was desired. After four days of the above treatment the aneurism was found to be diminished to half its former size; harder, but still pulsating. This result was certainly satisfactory, especially the commencing hardness, which indicated that coagulation was taking place within the sac. Pulsation of the superficial femoral artery, between the popliteal and the point from which the profunda femoris comes off, had been so far stopped that it could scarcely be felt. This again was in favour of cure, because the femoral artery, not being distended to its natural size, would contract and interfere with the transit of so great a quantity of the blood-current to the aneurism. Very little pressure with the finger upon the femoral artery below the profunda at this time arrested the pulsation in the sac. Two clamps were, therefore, placed over the vessel; two being used in order that one might every now and then relieve the other, and the patient be less inconvenienced than had one alone been continuously applied. The pad over the aneurism was to favour the emptying of the sac of fluid blood. The result is that the patient has been cured by most simple means (first suggested by Mr. Ernest Hart) of a dangerous malady, without recourse having been had to the operation of tying the femoral artery, an alternative which is not unattended with considerable danger to the patient's life.

Having observed how change of direction of the arterial trunk acted as a curative means in this case by diminishing both the current and its force, let us see if there are not other kinds of cases where the same simple method of relief may be applied. Probably all the structures of the body only perform their functions in perfection as long as they retain their normal direction and position; but this rule applies especially to arteries. In cases of hemorrhage from the hand occurring from several points simultaneously, you will find that if the hand be forcibly flexed on the forearm, the flow of blood in the radial and ulnar arteries will be so much arrested that the hemorrhage will cease; or, as that flexion, if long continued, is painful, raise the arm vertically from the shoulder, and the hemorrhage will equally cease. The subclavian artery describes a curve normally with its convexity upwards and outwards over the first rib. When the arm is extremely elevated, the convexity of this curve becomes directed downwards and outwards, and the free flow of blood through the vessel is interfered with. I remember a case of hemorrhage from the hand where styptics, the actual cautery, pressure, and other means had failed to arrest the bleeding, and where it was thought that either the radial and ulnar arteries or the brachial would have to be tied. As a last resource, the man was placed upon a bed, his arm was elevated over his shoulder, and the hand tied up to the bed-furniture in that elevated position. The hemorrhage shortly ceased, and the patient did well. Therefore, I may reccommend to your notice as a powerful therapeutical agent, in certain cases of hemorrhage, a change in the direction of the arterial trunks.—*Lancet, Feb.* 24, 1866, *p.* 195.

58.—ON A CASE OF WOUNDED ULNAR ARTERY, FOLLOWED BY SLOUGHING AND SECONDARY HEMORRHAGE, AND REQUIRING AMPUTATION.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

[The ulnar artery was wounded by a cut and the hemorrhage arrested by a pad. Secondary hemorrhage ensued. The wound was opened out and the artery tied above and below the source of hemorrhage. Sloughing, however extended far up the arm, the radial artery and the tendons of the front of the arm also sloughing. As there was no hope of regaining motion in the hand, and the case assumed so serious an aspect, amputation was performed above the elbow joint.]

After a short time pyæmia supervened. We looked out anxiously for the local manifestation, by symptoms, of the general diseased condition. No one can foretell whereabouts the secondary effects of pyæmia will occur; so it behoves you, in every case of pyæmia, to watch the patient, in order to detect as soon as possible the occurrence of any local mischief. Inflammation of kidneys occurred (probably of the left for he had pain and tenderness in the left loin on pressure), giving rise to blood and pus in the urine during many days.

You will inquire, How was he treated during all his time? Well, two things are essentially necessary in pyæmia: *stimulants*, to prevent the deteriorating influence of the pyæmic condition of the blood from so depressing the heart that the patient dies from failure of the heart's action; and *abundant nourishment*, so that the blood, from which all the tissues of the body have to be sustained and repaired, may be renewed by its appropriation. The principle regulating the constitutional treatment of pyæmia is plain and simple. The local symptoms, however, must be promptly and accurately attended to; all local collections of pus should be at once let out, &c., &c.

To return to our case. The man had secondary abscess near the shoulder-joint; it was opened above and anteriorly, but it was incompletely emptied by the incision, and the man suffered fresh pyæmic distress. I tried by continued exact pressure to keep the abscess empty; still the pyæmic distress continued, and the man's life was in great danger. I then made a deep and large opening into the abscess at its lowest part at the back of the axilla; a quantity of stinking pus was let out, and the pyæmic symptoms diminished immediately. The man still continues to improve, and, so far as I can see, now seems free from danger. Observe that the diet and all other influences were the same both before and after the abscess was opened the second time. Does it not therefore seem reasonable to suppose that the opening of the abscess at the most depending part, and the allowing of the putrid pus to drain away as soon as formed, explain his marked improvement? Undoubtedly. Yet, why so? What is the explanation? My idea is that the unhealthy matter in the abscess was being absorbed, and by poisoning the blood gave rise to what is termed constitutional hectic. I venture to reiterate the opinion which I have oftentimes expressed in my systematic lectures on Surgery, that the pathological cause of hectic fever, associated with chronic abscess, or following its being opened, is absorption of unhealthy fluid engendering pyæmia. On the other hand, it is right I should tell you that others explain this hectic condition by supposing it to depend on some ill-defined nervous morbid influence exerted primarily by the ganglionic system of nerves which induces the general disturbance.

At any rate we may learn from this case that when a collection of fetid matter is confined within the body, causing general disturbance, it should be at once let out by an opening made into its most depending part.—*Lancet, Feb.* 24, 1866, *p.* 197.

ORGANS OF URINE AND GENERATION.

59.—ON THE PROOFS THAT LITHOTRITY IS AN EMINENTLY SUCCESSFUL OPERATION.

By Henry Thompson, Esq., Surgeon to University College Hospital.

[The operation of lithotrity was performed successfully by Mr. Thompson, twenty-three times during the year 1865, and the mean age of the patients was sixty-two years.]

As stated at the conclusion of a preceding paper, I have operated by lithotrity successfully in forty-four *consecutive* adult cases during the last two years and a half, the mean age of the forty-four being also sixty-two years.

I still do not presume to adduce numbers sufficiently large to offer any absolute statement respecting the rate of mortality after lithotrity, or as to the result of any given method of performing the operation. But I think I may fairly lay claim to having established the following propositions :—

1. That lithotrity is capable of freeing elderly patients from stone at a very small risk, if well-constructed instruments, with delicate manipulation, be employed, and if watchful care be exercised in the management of the patient.

2. That the success of lithotomy has never been in any way comparable with that which I have been able to report as the result of the crushing operation.

I have only a few words to add relative to the mode of performing the operation. The principles enunciated in the paper of 1865, and also in greater detail in my work "On Lithotomy and Lithotrity," have been strictly adhered to—viz , to reduce to its minimum the employment of instruments of any kind ; to make each sitting as short as possible ; to pulverize the stone, or bring it to the condition of very small fragments, by the use of flat-bladed lithotrites ; to trust very much to the natural efforts of the bladder for the removal of this débris, and not greatly to artificial means.

Respecting the first condition, I may say that I have not in any single instance injected the bladder before operating, but have always used the lithotrite with any amount of urine which might happen to be present in the bladder, so that all instrumental manipulation, and all disturbance of the bladder by injecting, have been saved. As a rule, also, the lithotrite has been introduced once only at each sitting ; in very exceptional circumstances only twice. Neither the evacuating catheter and syringe, nor the double-current catheter, have been ever used.

Respecting the second condition, the lithotrite has rarely been permitted to remain in the bladder for more than one minute or a minute and a half at a sitting ; often only for a shorter period. In searching, near to the termination of the case, for a last fragment, two or three minutes may occasionally have been so occupied.

Respecting the third condition, I have very rarely found it necessary to use the fenestrated lithotrite more than once at the outset of a case in dealing even with a rather large stone. For subsequent sittings, and for smaller stones at the outset, the flat-bladed instrument, which is much less irritating to the bladder and pulverizes more completely, has been alone employed.

Respecting the fourth, I have removed more débris artificially than heretofore ; but not by means of the lithotrite, nor by washing out in the ordinary manner—that is, by a powerful injection of six or eight ounces through a large catheter, a method to which I entertain the strongest possible objection: first, as being almost useless to remove débris; and, secondly, as being exceedingly irritating to the bladder. But I have found Mr. Clover's new instrument very useful for removing such débris in those cases where patients have been unsuccessful or unduly slow in expelling it, and also in some instances where I have thought it advisable to remove the results of the crushing at the time of the operation. By its means a flux and reflux of so small a portion of water suffices to attain the object, and the action is so much more efficient than any other mode of washing out, that I cannot but regard it as an important addition to the appliances of lithotrity. It removes large and sharp angular pieces which would otherwise have troubled the urethra considerably in the act of performing their transit. At the same time it requires care, and not a little nicety in its application; and even then I consider it rather more trying to the patient than a single application of the lithotrite. As a rule, it appears better, therefore, to use it in those cases in which the patient is placed under the influence of chloroform; and then not only is the stone crushed, but the results may be removed at once, and little or no débris is passed until after the next sitting. In cases of atonied bladder, in which no urine can be passed by the patient's efforts, it is exceedingly valuable, since in these patients the bladder is so accustomed to mechanical contact, from the daily employment of catheters to remove the urine, that the organ will bear with impunity a comparatively protracted use of the lithotrite and of the injecting apparatus afterwards. There is certainly no case for the employment of lithotrity which I regard as so free from risk as that of an elderly patient who has lost all power to empty his bladder for years, and is consequently compelled to pass a catheter several times daily.—*Lancet*, *Feb.* 24, 1866, *p.* 200.

60.—ON A NEW INSTRUMENT FOR FACILITATING THE OPERATION OF LITHOTOMY.

By Henry Lee, Esq., Surgeon to St. George's Hospital.

[Sometimes considerable delay occurs in the seizure of the stone by the forceps after an opening has been made in the bladder. To obviate this difficulty Mr. Lee has devised an instrument which]

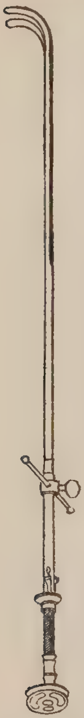
Consists of two blades like an ordinary lithotrite, but the lower blade is deeply grooved on the right side, the groove terminating abruptly half an inch from the commencement of the curve in the blades. During the operation, the stone is first seized and fixed between the blades of the "lithocrate." The instrument is then drawn as far forward as the separated blades will allow, and the patient is placed in the usual position for lithotomy. The ordinary lateral incision is then made, and the groove on the lithocrate directs the point of the knife to the prostate gland. The knife should be carried as far as the groove will allow. If the stone be small, the blades of the instrument will be separated to a small extent, and a small incision in the prostate only will be made. If the stone be larger, the lower blade of the instrument, in which the groove is made, will project further into the bladder, and a corresponding larger incision will be made in the prostate gland. After the incision is made in the perineum, the prostate is dilated with the finger, and pressed back over the stone at the same time that this is pulled forward. The stone is then removed entire through the opening in the perineum. Should the stone slip from between the blades of the lithocrate, it will be left within reach of the finger at the neck of the bladder, where it may be seized with the ordinary lithotomy forceps, or removed with a scoop. The advantages anticipated by this mode of operating are :—

1. The stone is seized before any incision is made.

2. The stone is held at the neck of the bladder, and may be there felt when the finger is introduced, and consequently the amount of dilatation required may be ascertained.

3. A straight groove, instead of a curved one, is presented for the incision in the urethra.

4. The length of this incision through the prostate gland is accurately determined : a small incision is made when a small stone is seized by the lithocrate, and a larger incision when the blades of the instrument are further separated by a large stone.—*Lancet, Dec.* 23, 1865, *p.*699.



61.—ON THE TREATMENT OF ACUTE ORCHITIS BY PUNCTURE AND COMPRESSION (IMMEDIATELY AFTER PUNCTURE) BY STRAPPING.

By Dr. W. Spencer Watson.

[Mr. Henry Smith, of King's College Hospital, about a year ago drew attention to the value of puncturing the testicle in cases of acute orchitis (*Retrospect*, vol. l. p. 183) Dr. Watson now offers a suggestion as to the combination of a further proceeding with, and in addition to, that of making a puncture.]

The first few cases in which I tried the plan of puncturing alone gave very satisfactory results, in almost all cases the patients expressing themselves as being relieved at once, *i.e.*, as soon as the escape of serum from the tunica vaginalis had relieved the tension of the parts, and it seemed to me that this escape of serum alone was the chief use of the puncture. I therefore tried the plan of puncturing the tunica vaginalis only, and letting off the serum by a grooved needle, and in one or two cases this seemed to give relief for the time; but under both these plans I soon found that in a few days the opening would close and the pain and tension return as before. Under such circumstances it was necessary to make a second puncture, but few patients would submit to a repetition of this proceeding, and I therefore found it necessary to adopt the plan which I have since followed, and with which I have every reason to be perfectly satisfied.

The plan consists in puncturing the testicle through the front or lower part, so as at the same time to let off the serum in the tunica vaginalis, and to relieve the tension of the tunica albuginea, and as soon as the bleeding has ceased, (the quantity which escapes being in all cases very small, seldom more than an ounce,) to apply a firm case of strapping, put on as tightly as the patient can bear it. At the same time I prescribe a full dose of opium, and direct it to be repeated twice or three times. As an instance of this plan of treatment, I may mention the following case :—

J. H., aged 20 years, came to King's College Hospital, under my care on May 8, 1865. He had had a gonorrheal discharge since Dec., 1864, for which he had been treated without injections. On the day previous to my seeing him he had been drinking freely, and the left testicle became suddenly enlarged and very painful and tender. The testicle was punctured and strapped immediately, and opium 1 gr. prescribed.

May 10. He was in great pain, but had been able to sleep the night before. Strapping was again applied and opium repeated.

12th. The strapping having become loose was again applied. He is now free from pain.

17th. The testicle is now very little larger than the natural size, and is free from pain or tenderness.

Thus in one week the acute symptoms of this case subsided, and the testicle was restored to very nearly its normal condition. In other cases the same result has been obtained at the end of a fortnight, and this is a much more rapid resolution than can be obtained by the ordinary method of treatment.

I am indebted to my colleague, Dr. Jno. Harley, for the suggestion of the plan of treatment by strapping; he having adopted that plan without a previous puncture; but I believe that the previous relief of tension materially assists in the restoration to health of the parts involved, and this more particularly where there is a considerable effusion of serum into the tunica vaginalis, for it is quite clear that when this is the case, no efficient compression can be made on the body of the testicle. It is also to be observed that after the escape of serum and blood, the size of the whole swelling is materially diminished, and the compression made will be proportionably effective, while the pain experienced by the patient will be greatly lessened.

As a further recommendation for this method of treatment, I may observe that should bleeding prove troublesome after the puncture, as I understand was the case in one case so treated by Mr. Henry Smith, this would be a most effectual way of restraining it. I have myself never seen more than an ounce of blood escape from the puncture, which I generally make with a Syme's knife, and most frequently not more than a drachm of blood is seen.—*Med. Times and Gazette, Nov.* 18, 1865, *p.* 563.

DISEASES OF THE SKIN.

62.—THE ACTUAL CAUTERY FOR NÆVI.

By J. R. A. Douglas, Esq., Hounslow.

[Mr. Douglas considers the actual cautery far less painful than nitric acid and other caustics, for the destruction of nævi in cases where the ligature is inadmissible or fails to arrest the vascular growth.]

Three cases lately cauterized by me illustrate well the use of this plan. An infant three weeks old I cauterized on the upper eyelid for a circular nævus, which was growing rapidly, and would soon have produced ptosis. Two applications quite arrested its growth; on the second, the nævus was raised by a tenaculum passed through it, which was burned down upon. Months have elapsed since the operation, and the only residue is a whiter mark than the skin where the vascularity was.

Another case was that of the smallest seven months' child I ever saw reared. A nævus appeared soon after birth at the extreme edge of one nostril, which, on examination, was found to pass up the orifice for some distance. At nine months old many applications of the nitric acid had been used, giving great pain; but the growth continued to increase. I applied the actual cautery freely to the nævus, passing it up the nostril, and using some momentary tension with the semi-button-like head of the curved instrument, to press well into the depression near the point of the nose. This has totally arrested the growth, leaving no deformity (so important in such a position).

The third case I shall trouble you with was that of a child a year old, with a large raised nævus of the middle of the cheek, with large varicose-looking vessels attached to it running back to the neck; the stain apparent through the cheek on opening the mouth. The nitric acid had twenty times been applied, the child crying for hours after its use. The ligature was thought inadmissible in London from the large surface that would be implicated. I applied a cautery half the circumference of the nævus in the middle of it; it apparently gave little pain, which soon ceased. After the surface had healed, I found it had so contracted that the same instrument covered the remaining part, which, having very florid edges, was spreading again. The second application has arrested all morbid growth; the vascularity gone; the large vessels almost imperceptible. I attribute all this to the known contraction of the cicatrices of burns. There is no distortion of feature, but a white irregular scar. Under these circumstances, I feel myself at liberty to urge the use of the nearly obsolete cautery in these cases. Such instruments are easily formed with the file from flat-headed bolts, nails, and screws, obtainable of every country ironmonger.—*Lancet, March* 24, 1866, *p.* 328.

63.—ON THE NATURE AND TREATMENT OF CHILBLAINS.

By Dr. Henry Samuel Purdon, Physician to the Belfast Dispensary for Diseases of the Skin.

Chilblains are an inflammatory state of the cuticle arising from exposure to cold, *Dermatitis congelationis*, as Hebra calls it. Now the specific inflammation (if I may so term it) that causes chilblains extends to the deeper structures of the corium, frequently to the cellular tissue beneath it, and is not primarily subcutaneous. In chilblains, like other inflammations, we have increased vascular action, followed by subcutaneous infiltration.

This affection is ushered in by a feeling of heat and itchiness, combined with redness and swelling of the affected part; the sensation of itching is aggravated by warmth. In some cases vesicles make their appearance, accompanied by a weeping of serum, being a typical eczema of Willan; in other instances by numerous fissures, the eczema fissum of Wilson, eczema rimosum of McCall Anderson, eczema fendill of the French. When the disease passes this stage, suppuration usually takes place, eventually ending in painful ulceration; indeed, cases have been recorded in which the integument covering the bone has completely sloughed off.

Chilblains are most frequently met with in children and females of a cold lymphatic temperament, and already several cases have been admitted at the Belfast Dispensary. This affection commonly appears on the fingers, toes, and ears, arising from exposure of these parts to cold when being imperfectly dried after washing.

Towards the prevention of chilblains in those who have been subject to them, bathing the hands or feet, as the case may be, in cold water, to which some vinegar has been added, and afterwards brisk friction employed, tends considerably towards keeping the parts in a healthy state.

In the majority of the prescriptions given by authors for the cure of this complaint rectified spirit enters largely. Tincture of arnica, on the same principle, by lowering inflammatory action and causing absorption of any effusion that may have taken place, is extremely useful in unbroken chilblains; but my favourite prescription is that of Marjolin's, and which seldom fails:—

℞. Balsami peruviani, ʒss.; spiritus rectificati, ℥iss.
Dissolve, and add
Acidi hydrochloridi, ʒss.; tinct. benzoini comp. ℥ss. M.

A little of this is to be rubbed occasionally into the affected part, or if preferred by the patient, a small piece of linen can be moistened with some of the above and applied to the part; a sensation of smarting is usually experienced for a few minutes.

When the chilblain is broken, I find the calamine ointment (Turner's cerate) a very good dressing, and if thought desirable to stimulate the part we may combine it with equal parts of elemi ointment.

If ulceration has taken place the nitrate of silver should be freely used.—*Medical Press, Nov.* 22, 1865, *p.* 456.

64.—CASES OF PRURITUS SENILIS.

By Dr. H. Beigel, Assistant Physician to the Metropolitan Free Hospital.

George Grant, a coalwhipper, 55 years old; six weeks ago he perceived an intense itching on both legs, particularly of the calves. He was obliged to scratch until the little pimples which had formed in great number on the skin began to bleed, which was followed by relief. Sleep made no difference, and sometimes he awoke covered with blood. In a few days the same condition had spread over the whole body. For some days he has now been free from itching, but as he gets warm in bed the sensation seems unbearable; the more he then scratches, the greater seems to be the itching.

Dec. 6, 1865. He was admitted an out-patient of the hospital. The skin bears marks of the scratching in every direction. Injection of one-fourth of a grain of morphia.

Dec. 8. No inconvenience after injection, but very great relief; for the first night, since the beginning of his complaint, he slept without being forced to scratch. Repetition of the injection.

Dec. 17. No recurrence of the former state; patient is perfectly free from irritation, and wishes to be discharged.

George Hyams, an upholsterer, 18 years old. has suffered for one month from an intense and very troublesome attack of *prurigo*, which, as in the former case, is tolerable at day time but unbearable during night.

Dec. 23. An injection was made with morphia (one-fourth of a grain) and no return of the itching has occurred since that day. This is the more remarkable as every practitioner knows how obstinate cases of prurigo generally are to every kind of medicine applied both through the stomach and locally to the skin. —*Medical Mirror, Feb.* 1866, *p.* 88.

65.—ON ECZEMA.

Mr. John Gorham, of Tunbridge, has tried the plan recommended by Mr. Erasmus Wilson, of treating the eczema infantum with minute doses of arsenic and one grain of calomel twice a week at bedtime, unless active diarrhœa be present. He uses at the same lint spread very thickly with zinc ointment; the lint being removed every morning, but *not the ointment*, which is allowed to cake on for a week, when it is removed with a spatula, generally exposing a nearly healthy surface. A fortnight's treatment is usually sufficient to reduce the case to a state that may be called convalescent.—*Lancet, May* 5, 1866, *p.* 503.

66.—TREATMENT OF ACUTE ECZEMA.

[Mr. T. P. Johnston, Assistant Surgeon H. M. 26th Regt. N. I., in a note to the Editor of the Lancet, gives the following treatment for allaying the itching in a case of acute eczema in a child aged nine months.]

I never find a lotion containing five grains of sulphate of zinc to the ounce of water fail to afford relief in cutaneous affections accompanied with intense itching. The pain it causes soon passes off. I should apply it with a camel-hair brush.—*Lancet, May* 5, 1866, *p.* 504.

DISEASES OF THE EYE AND EAR.

67.—ON EXAMINING THE FUNDUS OCULI.

Dr. Roseburgh, of Toronto, finds that the fundus of the eye can be examined without the mirror of the ophthalmoscope as follows. The lamp is placed on a table in a darkened room. The patient is seated on a chair two or three feet from the table, and facing the light. The chair of the observer is placed with the back against the table and between the patient and the lamp. When the surgeon is seated, the lamp should be immediately behind the right shoulder and very nearly on a level with the eye. By now bringing the right eye in a line with the lamp and the eye of the patient, but so as not to shade it, if the pupil had been previously dilated with atropine—the fundus is seen to be illuminated. In order to see the details of the posterior internal surface of the eye, a lens of about two inches focus is placed within an inch or two inches of the eye under examination. An inverted image of the fundus can now be seen forming an aërial image about two inches in front of the lens. The optic nerve entrance and the radiations of the retinal vessels can be seen by directing the patient to look at the light or two or three inches farther from the surgeon's eye, but in the same horizontal line.—*British Med. Journal, Jan.* 13, 1866, *p.* 38.

68.—ON THE TREATMENT OF GRANULAR OPHTHALMIA BY PRESSURE.

By Dr. William Stokes, Jun., Surgeon to the Meath Hospital and County Dublin Infirmary.

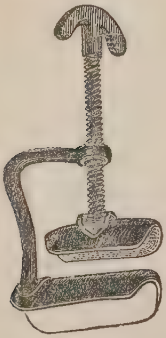
[The disease known as granular ophthalmia, as observed in Ireland, is of a more formidable type, and is more intractable to treatment than in England. The author of this paper wishes it to be understood, that he does not advocate this treatment to the exclusion of other methods which are adopted for the cure of this disease.]

Although, as far as I can determine, no mention of the treatment of granular ophthalmia by pressure, in any form, has as yet appeared in the literature of ophthalmic surgery, it has, nevertheless, been applied. In some of the Egyptian hospitals, as I have recently learned from a trustworthy authority, a favourite treatment for granular lids, which so frequently occurs as a sequela of the so-called Egyptian ophthalmia, is to strap down the eyelids by layers of adhesive plaster. This must produce a certain amount of pressure, which, most probably, accounts for the treatment proving so beneficial. I have also learned from Dr. Wordsworth, of the Moorfields Ophthalmic Hospital, London, during a visit which he paid to Dublin, last Autumn, that he has treated some cases of granular ophthalmia solely by the application of compress bandages, and with satisfactory results.

It was during a conversation with my friend, Dr. Robert M'Donnell, on the subject of granular ophthalmia, that it occurred to me that pressure would, most, probably, best succeed if applied to the granular surface of the conjunctiva palpebrarum, without, at the same time, causing any undue pressure on the bulbus oculi. Dr. M'Donnell had already in some cases of granular ophthalmia, inserted between the affected lids and ball of the eye masks made of glass and of the form of the ordinary artificial eyes, for the twofold purpose of preserving, if possible, the cornea from the mechanical irritation produced by the granulations, and also to subject these to a certain amount of pressure. Owing, however, to the difference in the convexities of the mask and the bulbus oculi the amount of pressure on the latter produced at the edges of the mask prevented the patient from bearing the instrument beyond a short space of time. The problem to solve, therefore, was to devise some means by which a continued pressure could be applied to the granular surface of the conjunctiva, without, at the same time, distressing the patient by any undue pressure on the bulbus oculi. After a number of experiments, the instrument which I devised, appeared to me, at first, to possess all the necessary requirements.

It consists of two highly polished ivory plates made somewhat thicker at the upper part corresponding to the reflected portion of the conjunctiva, at which situation the granulations are generally most abundant and of the largest size; at the lower extremity of the inner plate a very delicate spring, made of gold wire, was attached, terminating in a broad button, which made slight pressure on the anterior or convex surface of the ivory plate. The outer or anterior ivory plate is removable, there being no attachment to the broad button at the end of the gold spring. This instrument I found easy of application and capable of being borne by the patient without any difficulty. The inner plate being first introduced under the eyelid, and the anterior plate then being placed on the outer surface of the eyelid, was held in its place by the spring.

One disadvantage of this instrument was soon obvious, namely—the want of power to regulate the amount or degree of pressure to be applied. To be able to regulate this is of great importance in treating the disease in different stages of its development. To rectify this deficiency Mr. W. Pearsall, a pupil of the Meath Hospital and a skilful mechanician, devised and constructed a simple but ingenious modification of the first compressor I have described, and by which the pressure can be increased or diminished according as the surgeon thinks fit.



We may now proceed to consider briefly some cases of granular ophthalmia which have been treated by pressure in the Meath Hospital. The first of these was that of S. C., aged 19, who was admitted into the Meath Hospital on the 25th of last August. She had suffered from an acute conjunctival inflammation, three months previously; and, at the time of her admission into hospital, the conjunctival surface of both upper eyelids presented numerous and well marked examples of what are recognized by ophthalmists as pure papillary granulations. There was considerable impairment of vision in consequence of corneal opacity, depending partly on secondary keratitis, and partly on the well marked development of the superficial form of pannus, or pannus of the first degree, as described by von Græfe in his clinical lectures. In addition to these, there were several small ulcerations situated, for the most part, on the upper portions of the cornea.

I looked upon this case as one particularly well adapted for pressure, and the result showed that my anticipations were well founded. I commenced by applying the lid-compressor for one hour daily; and at the end of ten days, finding that the patient bore the instrument without any annoyance, and that there was a marked improvement not only in the affected portion of the conjunctiva, but also in the cornea, the pannus having to a great extent disappeared, I increased the time of application to two hours daily. This I continued up to the end of the fourth week. At this time the cornea and affected portions of conjunctiva began to assume quite a normal appearance, and an obvious improvement in the patient's power of opening the lids was observed.

The patient now could bear the instrument for four hours daily; and at the end of the seventh week I looked upon the case as one which had been brought to a successful termination. I did not, however, dismiss the patient from the hospital, as I wished to satisfy myself, if possible, that this signal improvement would remain permanent. At the end of three months the patient returned home.

The next case to which I shall allude is that of J. B., a young woman aged 24, who was admitted into the Meath Hospital on the 20th of last September. She had been under treatment in the country for some time previously, and the surgeon under whose care she had been, had repeatedly scarified the granulations, and followed up this treatment by the free application of caustics. This case, which at the time of the patient's admission into hospital presented, at and about the upper portion of the reflected conjunctiva, a large cluster of well-formed granulations, I treated by combining pressure with applications of a weak solution of nitrate of silver. At the end of three weeks the patient, at her own request, left the hospital. The improvement certainly was very great, and showed, even in a more marked manner than in the former case, how easily the instrument is borne.

The third and last to which I shall now allude, is that of C. H., aged eighteen, who was admitted into the Meath Hospital on the 12th of last October. This case presented an example of the highest degree of development to which pule granular trachoma may come. The palpebral conjunctiva of both lids were thickly studded with the vesicular or "sago grain bodies," which, in consequence of their form and semi-transparency, the German oculists have not inaptly compared to frog-spawn. The swelling of the conjunctiva, at its reflected portion, was especially remarkable. The cornea presented all the characteristics of an aggravated form of secondary ulcerative keratitis pannosa. At the time at which this report was being made (Dec. 9) the patient exhibits in every respect a most marked improvement both as regards the conjunctiva and cornea; and no treatment has been resorted to in this case except the application of the lid-compressors from two to four hours daily. These cases, few though they be, are sufficient to show that the treatment by pressure, applied in the manner I have described, can be adopted without discomfort to the patient, and without causing any irritation to a cornea, which is the seat of ulceration and pannus. The general improvement, in both conjunctiva and cornea, appears to proceed with more rapidity than it does under the ordinary method of treatment.

One great advantage which I claim for this treatment over the ordinary methods of scarification, and subsequent application of various caustics, is, that there is nothing in pressure which can tend directly or indirectly to the production of that most unfortunate, but too frequent result, not so much of trachoma as of its usual treatment, namely, entropium, with trichiasis. I have little doubt that this condition is more frequently the result of the use of caustics and the scarifying knife than the affection for which these are so frequently, and in many instances so injudiciously, made use of. Certain it is, that nothing can tend more to the development, of cicatrices in the conjunctiva, and consequent contraction of that membrane and inversion of the lid, than the injudicious use of those ordinary methods of treatment to which we have before alluded.

The treatment by pressure is also quite free from all danger of the occurrence of diphtheritic and other destructive forms of inflammation of the conjunctiva, which not unfrequently supervene after the treatment by inoculation.—*Dublin Quarterly Journal, Feb.* 1866, *p.* 38.

69.—THE TREATMENT OF OPHTHALMIA.

The treatment of gonorrhœal ophthalmia and of purulent ophthalmia of infants has undergone considerable change of late years. In place of the heroic treatment of former days—leeches by the dozen, low diet, tartarized antimony, &c.,—we frequently see only the mildest forms of treatment pursued, and apparently with greater success. At St. Mary's Hospital Mr. Ernest Hart has cases of both varieties now under treatment, which have been brought through severe attacks successfully by the persevering use solely of weak alum lotions and of the spray douche, with the local application of the *diluted nitrate-of-silver stick* to the mucous lining of the lids. The alum lotion is of the strength of three grains to the ounce ; it is to be used every half hour at first, and then every hour and two hours. In the case of a child the head of the patient is to be fixed between the knees, and the eyelids then drawn apart, and the discharge washed away first with lukewarm water, linen rags being used. The nitrate-of-silver sticks are diluted with nitrate of potash, as recommended, we believe, by De Graefe ; this localizes the action of the caustic, and limits it to the parts touched. In order to prevent the excess of action from superfluous caustic, Mr. Hart uses milk with a camel-hair brush immediately after applying the nitrate of silver. On the Continent it is usual to employ a solution of common salt, but this is in itself irritating. Mr. Hart learnt to use the milk from a suggestion of Dr. Horace Dobell, who applies it to the throat, and as a bland fluid it has many advantages over salt water, in the treatment of the eye more especially. The nitrate of silver usually needs to be applied every second day. This treatment Mr. Hart describes as very successful, and preferable to more heroic methods. The spray douche may be obtained by the use of the double tubes sold by perfumers for showering scented fluids. Many such have been devised. A particularly convenient form has been arranged by Dr. Andrew Clark. Those patients who cannot afford the few shillings must be content with ordinary cold douches applied every half hour for five or ten minutes in succession, a wet rag being applied in the interval.—*Lancet, Jan.* 6, 1866, *p.* 7.

70.—ON BLACK CATARACT.

By Haynes Walton, Esq., Surgeon to the Central London Ophthalmic Hospital.

[This form of cataract is almost unknown to the profession, except by a paper read before the Royal Medical and Chirurgical Society a few years ago, by Mr. Walton. By a naked eye examination no cause of blindness can be discovered; but by the ophthalmoscope it is seen that the lens is of a very deep colour. Whenever the cataract has been extracted in any of the few cases which have been met with it has always been of a deep brown colour.]

An actual black cataract has been lately removed by Mr. Walton, of which we shall give the particulars, as there are some points of physiological and pathological interest connected with it.

A man, 65 years of age, was sent to Mr. Walton by Mr. Philbrick of Colchester, because of his blindness. In both eyes there was the congenital defect, coloboma iridis, or deficiency of a segment of the iris, in a marked degree. His sight was so little affected by it that he worked successfully as a gardener, and his employers never knew that he had any defect.

The coloboma is sometimes accompanied with a split or fissure in the retina. It is impossible to say whether this existed; probably not, because of the quality of the sight.

Except that there were a few minute dots of grey in the pupil of the left eye, nothing could be recognised there, as all else was black. Even when the light of the ophthalmoscope was concentrated on it, nothing more could be seen; but as the interior of the eye could not be illuminated, sufficient light not entering for the purpose, it was evident that there was some black body that intercepted the rays. There was scarcely any sight in the eye, only the mere perception of light from darkness.

Mr. Walton operated by extraction, and made the lower section of the cornea, because the iris was cleft in that direction. The cataract proved to be quite black, and of the usual consistence. It was put aside for a minute examination, but unfortunately was destroyed. The grey markings spoken of must have been in the capsule, for no such appeared on the cataract.

The case proceeded without a bad symptom, and the cornea healed well, but there was no beneficial result. There is no more sight than existed before the operation. This important question then arises. Are those changes in the interior of the eye, by which the eye is spoiled, and the nature of which cannot be ascertained by the ophthalmoscope when the crystalline lens is lost, associated with the peculiar alteration in the lens? Do they go together as allied diseased states, or is their association accidental?

But a few years ago, every case of loss of sight, in which the pupil looked black, was called amaurosis. It is very likely that many a black cataract was so designated. This shows how necessary it is to use the ophthalmoscope, both for the positive and the negative information we get from it.

In the right eye also there was a cataract. The centre of it was quite black, while a small marginal portion was rather deep brown than black. The interior of the eye could not be illuminated by the ophthalmoscope. There was very little sight. Mr. Walton did not operate; he thought it most probable that the back of the eye was defective, as on the opposite side; and the patient was not at all willing for an operation.—*British Med. Journal, Jan.* 27, 1866, *p.* 91.

71.—ON THE TREATMENT OF STRABISMUS WITHOUT OPERATION.

By C. Holthouse, Esq., Surgeon to the Westminster Hospital and to the Surrey Ophthalmic Hospital, &c.

An essential preliminary to every rational mode of treatment, is a knowledge of the pathology of the affection with which we have to deal, and the more nearly we approach exactitude in this respect, the more likely is our treatment to be rational in its scope and certain in its results; I shall, therefore, premise the discussion of treatment by a few preliminary remarks on the pathology of strabismus, and the class of cases which are best adapted for tentative efforts at a cure without operation.

For practical purposes nearly every case of strabismus may be grouped under one or other of the two following divisions: 1. The nervous or paralytic. 2. The muscular, or non-paralytic; the loss of association in the movements of the eyes depending, in the first group, on a loss of innervation or paralysis of one of the ocular muscles; whilst in the second, the innervation is perfect, or may be in excess, but one or other of the muscles has undergone some functional derangement, or structural change. Of the nervous or paralytic strabismus, we are too little acquainted with the changes which take place in the nervous elements as a consequence of disease, to be able to say, in all cases, how the suspension of their functions is brought about; but reasoning from analogy, and from what we know of the causes which give rise to more general paralyses, as hemiplegia, we may assume that pressure plays a not unimportant part; and this may arise either from over distension or congestion of the bloodvessels, from extravasation of blood or serum, from inflammatory effusions, or from tumours, whether osseous, neuromatous, vascular, or of any other nature; and the action of these may be either on the brain, or on the nerves issuing from it in any part of their course. We have, therefore, to consider in the first place, whether the paralysis has a cerebral or a nervous origin; and, in the latter case, whether it is within the cranium proper or within the orbit; thus we may not unfrequently arrive at a correct estimate both of the locality and the nature of the mischief.

The larger proportion of cases of paralytic strabismus which fall under the notice of the ophthalmic surgeon, are not of cerebral origin, or at least there are no symptoms to indicate any such lesion, while of the nervous cases, rheumatism and syphilis furnish the most abundant examples. The following case, though narrated in a former work *(On Squinting, Paralytic Affections of the Eye, and certain Forms of Impaired Vision)* is worth republication from the bearing which it has on the foregoing remarks.

Case of Paralytic Convergent Strabismus, of Itracranial and Nervous Origin, from Syphilitic Periostitis of the Basis Cranii.—James P., aged 30, a compositor, applied to me at the Public Dispensary, Lincoln's Inn, March 6, 1847, with the following symptoms. He had a convergent squint of the left eye, and was unable to abduct it, or even to bring the cornea into a central position; he could, however, rotate it further inwards, as well upwards and downwards. The size and movements of the pupil were normal, and the vision of each eye, when examined separately, was good, though when both were open the patient experienced much confusion of vision. When told to put out his tongue, it was observed that the point deviated to the left, and that the two sides of the organ presented a remarkable contrast, the left half being atrophied, broad, flat, thin, and flabby, whilst the right was firm, plump, and roundish. The paralysis of half the tongue was attended with considerable inconvenience in mastication; but produced no appreciable effect on the patient's articulation. Sensation and taste on the paralysed side were perfect. The patient complained of constant pain in his head and at the back of his neck. The above symptoms first made their appearance in July of the preceding year, and had hitherto resisted the treatment adopted. When he applied to me, I recognised him as a patient who had been under my care some months previously to the present attack, for syphilitic periostitis of both tibiæ, and coupling this fact with his present symptoms, I concluded that they arose from a similar disease affecting a portion of the basilar process of the occipital bone or its periosteum, and implicating the sixth and ninth nerves at their origin.

Under the influence of antisyphilitic treatment this patient was quickly relieved of the pain in his head and neck, and slowly recovered power over the external rectus. The paralysis of the left half of the tongue was more slow in disappearing, so that on July 12th, 1847, when he again consulted me for a slight return of the head symptoms, the tongue had undergone no alteration, but the strabismus was less marked; he was able to move the eye slightly beyond the central position, and to maintain it in the centre of the orbit, though of course the sound eye then became inverted. Six years afterwards, when I again saw this patient, all traces of the strabismus had disappeared, and a scarcely appreciable difference could be discerned between the two sides of the tongue.

This case is a good example of complete recovery of the external rectus of one eye from a paralysis of many months' duration, and which led to no shortening of its antagonist, or to any implication of the adductor muscle of the opposite side. Both of these results may follow a paralysis; so that when the latter is cured, either of them as pointed out by Von Gräfe, may lead to an ordinary concomitant squint.—*British Med. Journal, Feb.* 17, 1866, *p.* 173.

72.—ON ATROPINE POINTS.

By J. Z. Laurence, Esq.

The use of *solutions* of atropine in ophthalmic practice is attended with certain practical inconveniences. Mr. Streatfield proposed to substitute disks of thin paper or gelatine, steeped in solutions of atropine. A disk is placed on the eye; the tears then dissolve out a sufficient quantity of the atropine to dilate the pupil. These *dry* preparations reccommend themselves by their simplicity and portability.

I have lately had made for me by Mr. Curtis, of Baker Street, pencils or points impregnated with atropine. These atropine points are, like nitrate of silver ones, adaptable into wooden holders, and may be carried about in the waistcoat pocket. If the atropine-point is lightly passed, once or twice, over the conjunctiva of the lower eyelid, a sufficient quantity of atropine is washed off to dilate the pupil for ophthalmoscopic or other purposes.—*Ophthalmic Review, Jan.* 1866, *p.* 426.

73.—BLUE GLASSES.

Nothing strikes the visitor more, not at the Clinique only, but in Berlin generally, than the extraordinary number of persons wearing blue glasses. These, mostly plane, and ordered for the purpose of protection from the yellow rays of light, are now frequently preferred of the globular shape. The neutral tint or smoke glass is seldom seen. The sunlight, throughout the year, is more glaring abroad than we have it here. Moreover, there is at Berlin, besides Von Graefe, an authority perhaps greater than he in this particular, favouring the resort to blue glasses, Professor Böhm, who has made the merits and effect of the blue light his special study, and to whom practical ophthalmology is certainly indebted for valuable information on this head. Whoever has cared to enquire into, and in suitable cases try, the effect of blue glasses, will not, on account of a possible excess due to fashion, too unfavourably judge the ascendancy obtained by this therapeutical agent.—*British Med. Journal, March* 24, 1866, *p.* 308.

74.—TREATMENT OF MALIGNANT AND RECURRENT TUMOURS OF THE ORBIT.

During the last few weeks we have had under notice a somewhat novel treatment for the more complete eradication of these obstinate and distressing affections; and, as far as we are able at present to form an opinion, the results seem most promising. The treatment appears to be applicable, not only to the malignant growths within the orbit, but also to those forms of recurrent fibroid tumour which spring from the periosteum of the orbital bones, and recur almost as rapidly as they are excised.

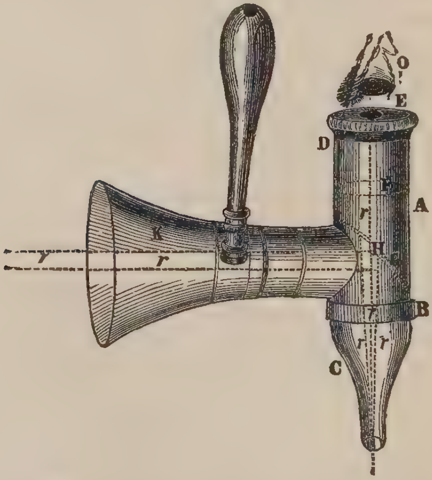
The first case was under the care of Mr. De Morgan, the second under Mr. Hulke, both of the *Middlesex Hospital*, and the third under Mr. Geo. Lawson, of the *Royal London Ophthalmic Hospital, Moorfields.*

The treatment consists in rapidly excising the whole of the growth from within the orbit, removing, as far as practicable, all the tissues within the bony cavity as far back as the optic foramen, and then freely applying the chloride of zinc paste spread on strips of lint to the bottom and sides of the orbit, so as to effectually destroy any germ of the disease which may have escaped the knife. In two of the cases, the actual cautery was used to the periosteum lining the orbital walls previous to the application of the chloride of zinc, as there still remained small adherent particles of the disease, which it was difficult to get away with the knife. No untoward symptoms followed in either of the three cases; the pain was not more severe than after an ordinary cutting operation; and each of the patients made a rapid recovery. In the first case, more than twelve months have elapsed since the disease was removed; in the second, more than six months; and in neither have any recurrent symptoms appeared. The last case is still under treatment, the operation having been performed only about three weeks ago. Each of these three patients might have been fairly considered most unfavourable for any surgical treatment, as in two the disease had been twice removed and had returned, and in the third the tumour had attained a size which it seemed almost rash to attempt to meddle with. We shall at some future period take an opportunity of reporting these cases in detail, when more time has elapsed to justify a positive conclusion as to the beneficial results of this mode of dealing with these terrible affections.—*Lancet, Jan.* 6, 1866, *p.* 7.

75.—A NEW OTOSCOPE OR SPECULUM AURIS.

By Dr. John Brunton, M.A.

In the spring of 1861, while examining a patient's ears with the ordinary aural instruments, two serious difficulties arose to me in forming a correct diagnosis with such instruments—viz.: 1. That the observer's head very greatly obstructed the light; 2. That the eye could not get near enough the object to permit of minute examination, and more so if *sunlight* instead of *artificial* light was used. The instrument of which a description and drawing are here given then suggested itself to me.



The instrument consists of a brass tube (A), two inches long and three-quarters of an inch in diameter, to one end of which (B) is made to fit on, by sliding, an ear-piece similar to Toynbee's aural speculum. At the other end (D) is an eye-piece (E), with a lens (F) of moderately magnifying power ; the eye-piece slides so as to admit of focal arrangement to suit the eye of the observer. In the body of the instrument, near the ear-piece end, and set at an angle of 45°, is a concave mirror (G), with a hole in the centre (H) ; this aperture in the mirror is in the line of the axis of the tube and ear-piece. At a right angle to the body of the instrument, and opposite to the mirror, is adapted a sliding funnel-shaped polished silver reflector (K) for collecting and concentrating the rays of light, so that the rays (*rr*) are let in at the side, and, falling on the mirror (H), are reflected and concentrated into the ear, and carried back (*r′r′*) to the eye of the observer (O) through (H), the hole in the mirror, and are magnified by the lens of the eye-piece (E, F). There is also a handle attached to the light-reflector, which works on a sliding band, and can be turned about to suit either hand of the observer as he may wish, when looking at the right or left ear.

With this instrument the observer's eye is brought in close contact with the patient's ear, and the light coming in at the side is in no way obstructed. The otoscope is easily used, and with the best advantage by sunlight. It is applied thus : With the left or right hand, as the case may be, the surgeon pulls directly outwards the auricle, inserting the ear-piece into the external meatus. He then desires the patient to turn his head round till the light falls directly upon the mirror, when the meatus, membrane of the tympanum, &c., will be observed very clearly, the smallest change of structure may be noted, and the movement of the membrane readily seen. If the membrane and ossicles are gone, the tympanic cavity can be observed and noted. With this instrument the blood-vessels that traverse the membrane of tympanum can be seen in the healthy ear.

The advantages of the instrument are—

1. Simplicity of construction.
2. Ease of application, a few trials sufficing to make the observer expert.
3. Precision and minuteness with which the ear can be examined.
4. That it can be used with artificial or sun light (the latter preferred).
5. That it can be used with the magnifying power or not, at pleasure.

The instrument was made for me by Mr. White, of Renfield-street, Glasgow, optical and mathematical instrument maker to the University. It was shown some time ago to the Medical Society of London. It is used in Glasgow.

The principle of the instrument can be adapted in many ways. I have used the otoscope with advantage in examining the nasal passages.—*Lancet, Dec.* 2, 1865, *p.* 617.

76.—ON A NEW METHOD OF APPLYING REMEDIAL AGENTS TO THE CAVITY OF THE TYMPANUM.

By Dr. EDWARD BISHOP, Surgeon to the Metropolitan Infirmary for Diseases of the Ear.

[One of the most valuable practical teachings of modern surgery is the recognition of the fact of how much may be done within the cavities of the body. Great care in manipulation is necessary in applying anything to so delicate and sensitive a part as the tympanic cavity.]

The method I am adopting at the hospital is to apply lotion containing such soluble remedial agent as may be considered appropriate, by means of tepid pulverised water. It is evident, as far as the *principle* is concerned, lotion of any strength, up to the point of saturation, may be used. The quantity required to come into contact with the whole lining membrane is so small that there is not the least fear of mechanical injury, and by slightly turning the point of the instrument, the pharynx, the nasal passages, and the parts connected therewith, may all be subjected to the action of the remedy—an important fact, as it is found that the whole tract of mucous membrane lining these parts is generally suffering from the same morbid condition. As the pulverised lotion is driven off in the finest state of subdivision, it may be sent into the cavity of the larynx; but on this point I have not yet had much experience, and therefore refer to it with diffidence. In the treatment of ozœna, however, it will be found useful, as a solution of such agents as carbolic acid or creasote may be sent into every crevice and cranny of the tortuous nasal passages and the parts connected with them.

The apparatus consists of a small graduated glass syringe similar to that used for subcutaneous injection. The solution is gently forced guttatim into a cylinder, and at the point where it leaves the nozzle of the syringe it is caught by a current of air sent by a pump worked with a proper degree of force by the hand. This drives the fluid forward in a pulverised state. This small apparatus is then attached to an ordinary Eustachian catheter previously introduced, and suspended by a suitable apparatus. To ensure complete pulverisation, the end of the catheter is covered by fine gauze wire.

Some amount of dexterity is required in the successful use of the instrument, but this is soon accomplished by any one accustomed to the introduction of the catheter.

It is equally applicable to those cases where it is thought desirable to use Politzer's catheter, introduced a short distance only within the nasal passage—the pulverised solution finding its way through the Eustachian tube into the cavity of the tympanum during the act of swallowing.

The instrument described above has been made for me by Weiss and Son, Strand, London. I am conscious it is capable of much improvement, and in the hands of those highly scientific gentlemen has already been put into a *more* practical form than I at first anticipated.—*Med. Press and Circular, Jan.* 3, 1866, *p.* 9.

77.—ON DIFFICULTY OF HEARING IN CASES OF PERFORATION OF THE MEMBRANA TYMPANI.

By Dr. F. E. Weber, Berlin.

In order to formulate my statements let us imagine—

1. A person with a large perforation in the membrana tympani, not of long standing, which has entirely passed off, without thickening of the mucous membrane lining the tympanum; one, further, whose ossicula are not yet injured, and the rest of whose acoustic organs are intact. But let us suppose the tube to be closed, proving on examination to be completely obstructed by catarrh, and incapable of being cleared either by the experimentum Valsalvæ or by the air-douche aided by a swallowing movement, so that the probe cannot pass the stricture without resistance. Let us imagine further, that a watch which under ordinary circumstances can be heard at a distance of thirty feet, is in this case not audible by the patient beyond a range of two feet, vocal utterances being equally inaudible. Now let us suppose a few days to have elapsed, and the inflation produced by the catarrh to have somewhat subsided, so that the tube on examination, though still charged, seems already somewhat less swollen, proving on the attempt to swallow to be uniformly filled with mucus, the range of hearing being, say three feet. If now the entire bore of the tube—the cavity of the tympanum having previously been found quite free and clear—be at this moment opened by means of a bougie of suitable size, which is allowed to remain a few minutes, and an air-douche be then passed through, so that the noise of the air passing through the mucus is distinctly heard by aid of the otoscope, the patient, after this opening of the passage, will hear the watch at a distance of from twenty-five to thirty feet.

2. Now let us take another case in which, along with the defect in the membrana tympani, further derangement has resulted from chronic inflammation of the middle ear, such as sclerosis of the mucous membrane, or thickening of the coatings of the fenestræ, or it may be even that, through a slowly-progressing process (antecedent perhaps to the inflammation of the cavity and the rupture of the membrane) secondary changes have passed over to the auditory nerves. Supposing such a patient, while the tube remains closed, to hear the watch at a distance varying, according to circumstances, from about half an *inch* to a few inches, he will, after it has been opened, perceive the ticking at a distance of one *foot*, or at most of a few feet.

These results, so far as regards the general drift of these remarks, occur with such marked uniformity, that hitherto, amongst some hundreds of cases carefully examined in reference to the point in question, I have only found exceptions in two instances of highly tuberculous individuals.

2nd. A further highly important and interesting observation stands in close connexion with what precedes—namely, that in these cases of subsided chronic inflammation in the cavity of the tympanum with perforated membrane, the opening and restoration to a normal condition of the passage of the tuba Eustachii produces in most instances an equal improvement in the power of hearing—the effect being in few cases less, more frequently greater—to that which ensues on the application of the so-called artificial ear-drum.

Definitive observations on the nature of this coincidence I have as yet not made, still (seeing that the application of the artificial ear-drum is relatively but seldom effectual, whilst the opening of the tube is a remedy which nearly always affords relief) the agreement between the results which ensue on the employment of the several remedies will be found mainly where there is a wider range of hearing, whilst in extreme cases of difficulty of hearing, arising in the manner referred to, the remedies cease to be equal in their effects, or even to produce uniformly, when combined, the sum of their separate effects.

I have endeavoured to arrive at an explanation of this striking fact; but even from the most distinguished physiologists who have occupied themselves with investigations of the organs of hearing, with whom I have entered into correspondence on this subject—namely, Professor Helmholz, of Heidelberg, and Professor Henle, of Göttingen, I have failed to receive such an explanation; nor have I been able to discover the cause why the Eustachain tube should prove to be of such eminent importance where the membrane of the tympanum is wanting, as used *only* to be assigned to it where that organ is preserved.

What I surmise in reference to these striking phenomena—though my views require for their confirmation the collection of yet further experiments and observations—is this, that the tube acts in these cases in the manner of a sounding-board, or rather as a resonant pipe, in connexion with the cavity of the mouth. This idea, founded on investigations and experiments which are reported in the celebrated Joh. Müller's Physiology, I have also communicated to Professor Henle. The reply I received was, that he could no longer think of advocating this opinion, since he had learnt from dissections that the parietes of the Eustachian tube, from the isthmus down to the orifice in the throat, ordinarily (except during the act of swallowing) lie flat together, so that there can hardly be any question of resonance or resonant connexion with the cavity of the mouth; we might, he adds, rather explain the phenomena in question by supposing that, in the particular cases referred to, the tube attains a physiological importance which it does not ordinarily possess.

I could never share this view, because the state of the organ in the dead body by no means proves that the same applies also during life, when the vital tension of the parts of the organ and the various degrees of pressure to which they are subjected alter the mutual relations of the parts.

I have been accordingly most agreeably surprised in this respect by the results of some quite recent labours in reference to the Eustachian tube, published by the prosector of the University of Munich, Dr. Rüdiger, which prove that *even in the dead body*—contrary to the received opinion—the smaller upper part of the bore of the tube is always open through its entire length, and that therefore there is an unbroken column of air in communication with the organs of respiration. One chief objection to my explanation is thus removed.

The effect of the artificial ear-drum has also been explained by some on the principle of resonance; whilst others think it must be attributed to a pressure which the artificial drum (of Toynbee) or the pellet of wadding (of Yearsley) exerts upon the remaining fragments of the tympanic membrane, or of the ossicula auditus, and thereby mediately upon the labyrinth.

Much may be said in favour of each of these explanations, and much also against their exclusive adoption. Thus, for instance, the artificial drum proves effectual in cases where there can be no question at all of the pressure above alluded to; whilst, on the other hand, the phenomena of resonance in most cases predominate greatly; and during the *application* of the little instrument in question patients chiefly complain that the sound hums and *reverberates* too much, and that every noise is heard as if proceeding from a hollow cask.

Well, then, this resonance theory, which, though it may be open to objections, is alone applicable in both cases, explains why the two remedies, differing so widely in their physiological and therapeutic effects, produce nevertheless interferences which frequently coincide.

Now, without dwelling further on the manner in which this conception of the mode of operation of the artificial drum harmonizes with the account which Toynbee so ingeniously gives of the normal membrana tympani (according to which the drum with its ossicula, like the iris in the eye, must be regarded only as a kind of adjusting apparatus for the correct perception of the highest as well as the lowest sounds), I am of the opinion that it is reserved for further investigations to arrive at a correct solution of the problem.

3rd. Meanwhile, the physiological fact already established leads to a still more important practical application in therapeutics. Whilst I acknowledge how, to the praise of its inventor, the so-called artificial drum produces an almost magical effect, yet everyone familar with the subject will admit that in the majority of cases, often quite contrary to expectation, the magnificent effect fails to ensue on the application of that little instrument; that its use is, under any circumstances, for many patients very disagreeable, and in unskilful hands affords a doubtful relief—nay, is even prejudicial; that it is only when the patient or the physician *accidentally* hits upon "a certain" right spot that improvement in hearing ensues; that all sounds and noises are heard frequently with disagreeably loud reverberations and hummings; and that, especially in the hands of not very cautious laymen, its constant application for a series of years always, as an irritant, as a foreign body, operates prejudicially upon the exposed mucous membrane of the tympanic cavity and the edges of the membrane, and thus reproduces and keeps up the state of suppuration. I accordingly only employ it as an occasional resource, whilst habitually the *methodical* treatment of the tube, with the object of *restoring its passage to as normal a condition as possible*, by means of bougies (often combined with injections of astringents and nitrate of silver, and with treatment of the mucous membrane of the tympanic cavity and the throat) effects *everything*, often more than any other means, and *always more harmlessly*, and of course affords a more constant uninterrupted relief than can be rendered, in the majority of cases where the tympanic membrane is ruptured, for the merely *temporary* heightening of the power of hearing, by means of the so-called artificial drum.—*Lancet, Feb.* 17, 1866, *p.* 173.

SYPHILITIC AFFECTIONS.

78.—ON SYPHILITIC INOCULATION IN 1865.

By Henry Lee, Esq., Surgeon to St. George's Hospital.

Much has been said and written as to the identity or plurality of venereal poisons. Observers at one on the different nature and properties of the soft and indurated sores are at variance as to the identity or duality of their cause. To different minds the same array of facts present a different view, and carry opposite conclusions. The pathologist, in his study of morbid causes, encounters the same difficulties that divides the schools of naturalists—for instance, the Darwinians and anti-Darwinians.

It is foreign to the purpose of this lecture to enter into the question of the unicity or duality of the syphilitic poison ; but it is important to remember that the natural and artificial inoculation of the skin or mucous membrane of a healthy individual with the virus of what we will term true syphilis produces a characteristic affection locally and constitutionally, and does not induce a local, soft, suppurating sore with any concomitant suppuration of the neighbouring glands. The apparent exceptions to this rule are so very rare that we may fairly question their reality. Dr. Danielssen's experience of the inoculation of lepers afforded confirmation to his mind of the long-established axiom—that the soft chancroid does not affect the system, and consequently does not produce general syphilis, for among the many thousand inoculations he had witnessed he only noticed *one* exception, and this exception was afterwards perfectly explained by the discovery that after nearly four hundred inoculations of a leper with the virus of the soft sore, without producing constitutional manifestations, an indurated chancre, followed by unmistakable signs of secondary syphilis, was the result of the accidental inoculation of the same leper with secretions obtained from an indurated chancre. Again, the same body of men—of soldiers, say—have before now contracted the local soft chancre at one station, and the indurated one with constitutional syphilis at another station.

What we find true of the many we discover likewise in individual cases. The same patient may have suffered from two, three, or six attacks of the soft sore without secondary infection ; but when he has contracted the indurated chancre then secondary infection has manifested itself. The modern history of syphilis points the same way. The widespread outbreak of syphilis at Rivalta and the inoculations performed from time to time with the view of proving the possibility of inoculation with the products of constitutional lesions were, in all cases, preceded by an affection *corresponding to the descriptions* of the *indurated sore and glands, and not to that of the soft sore.* But, it will be asked, what are the characters on which we rely for our distinction of the two affections? Shortly these :—

The simple, soft, local venereal ulcer is marked by a process of active ulceration and suppuration, attended with inflammatory phenomena. Neither the ulcer nor its cicatrix presents any well-defined and strictly limited induration; it discharges a purulent secretion which readily inoculates the neighbouring parts, and the ulcers are often multiple or become so. If inguinal adenitis ensues, it is confined to one gland, which tends to inflame and suppurate. The virus of this sore can be *invariably* inoculated on the same subject *through several generations*, always producing effects *essentially like the original ulcer.* The disease manifests itself two, three, four, or rarely five days after exposure to contagion; one attack conferring no immunity whatever against subsequent ones.

The indurating, infecting, or true syphilitic chancre, is characterized by the adhesive form of inflammation; it is more or less ulcerative in character, but not by any means invariably so, the specific induration being sometimes formed *à froid* without inflammatory phenomena. This sore is generally solitary, or, if multiple, it is so from the first, the base and periphery of the lesion possessing a well-defined but variable amount of induration. The concomitant adenitis is generally symmetrical, consisting of a chain of enlarged indurated glands, or indurated lymphatic vessels, and without any active inflammatory or suppurative action in them. The lesion appears from between five days to five weeks after contagion. The discharge from the indurated chancre is not auto-inoculable, except under certain conditions of *attendant irritation;* and then the inoculation never produces another indurated ulcer, but a pustulation, with some erosion or trifling ulceration at the point inoculated. The indurated chancre is protective against subsequent attacks, conferring a *relative*, although not an *absolute*, immunity for the future. Constitutional syphilis no more frequently repeats itself in the same subject than other diseases depending on animal poisons, such as the contagious exanthemata.

I have purposely described the most typical and common form of these diseases, as illustrated by the cases just given, and avoided the more anomalous, mixed, and non-conformable cases, in order to fix the attention upon the points of contrast between these two forms of venereal lesion.

An examination of the edges and base of a venereal ulcer will generally indicate its character. In the local sore these are *discontinuous*; the vertical edge, as it joins the base, is a little undermined. You can move the former laterally a little on the latter, and a little pus will ooze out. The base is also cellular, honeycombed-looking; and on raising the ulcer between the finger and the thumb you will find it quite soft, or doughy it may be. In the other form it is quite different. The edges slope inwards to the base, and join it. In other cases the chancre is a flattened induration, or an enlarged papule, covered, perhaps, with an adherent scale. The indurated ulcer on the prepuce rolls over *en masse* as the part is retracted; the soft one bends on itself. Again, a shining, gummy exudation covers the dull-red surface of an indurated sore at first, and in time this surface becomes dotted with little points of molecular disintegration; while the local soft lesion is either a pustule at first, or an erosion, which speedily becomes an ulcer, painful, and discharging pus freely on pressure.

The character of a chancre at one time need not, of course, be its character at another; nor is the absence of induration a proof of its local nature. Before deciding on that, it would be necessary to examine into the attendant signs, the state of lymphatics and glands; but when a specific induration can be detected in the chancre, with a similar state of the neighbouring glands, the *positive* evidence becomes so reliable *that you may safely and certainly prophesy the subsequent evolution of a syphilitic manifestation in the subject of it.*

We now approach the subject of *inoculation.* It is well known that the virus of the soft sore is not only auto-inoculable, but the inoculations may be propagated through several generations. The secretion of the soft sore is invariably that of pus, which forms the vehicle of the virus. The ordinary secretions from the indurated sore are not inoculable in the same individual. Originally Ricord maintained that a person could only have constitutional syphilis once; that the diathesis did not double itself in the same constitution.

In 1853, I showed that the indurated infecting sore, in its origin, was attended with the adhesive form of inflammation, and was therefore an essentially different morbid process from the soft, local, suppurating sore. In 1856, I asserted the non-inoculability of the indurated chancre upon the patient himself, supporting the doctrine by the publication of the cases in which I had made this the subject of experiment; and subsequently in 1858, in continuation of these observations, I pointed out the exceptional circumstances under which such inoculations might succeed.

The correctness of these observations received abundant confirmation at the hands of a large number of Continental and British surgeons. The latest writer on the subject of venereal disease, Dr. Hammond, a man thoroughly accustomed to scientific observation, speaking to his pupils, says: "I have, as you know, endeavoured very many times to inoculate the virus of an infecting chancre on a syphilitic person, and never once with an affirmative result............Try it for yourselves—and most of you have already done so—if you entertain any doubt, and you will be satisfied."

On the other hand, Drs. Boeck and Bidenkap assert that they not only employ, but prefer, the secretion of the hard sore, as more effective than that of the soft in the process of syphilization.

The experiments on the two sides have been numerous: those of the first set of observers have been indeed so numerous that the question was considered a settled one until the method of treating constitutional syphilis by "syphilization" was adopted. Dr. Marston tells me that the result of a great many artificial inoculations performed in the Military Hospital has been, that they have *never* failed in getting an affirmative result with the virus of the soft sore; while they have *generally* failed with that of the hard in producing any effect upon the same individual. In one instance lately he employed the purulent secretions from the two forms of sores at the same time upon the same patient. He was careful to use different lancets, *because he holds it to be essential, when dealing with an agent possessed of such peculiarly penetrating and irritating properties as the virus of the soft sore*, to guard against the presence of even the minutest quantity of it. He succeeded in propagating a lineal series of ulcers, which left large cicatrices on healing, from the pus of the soft: but only two generations of pustules with that obtained from the hard sore.

Assuming the statements of Drs. Boeck and Bidenkap to be perfectly correct as to the inoculability of the hard sore in a syphilitic soil, we are brought face to face with the question, How can the differences between the results obtained on the continent, in England, America, and Italy, be reconciled with those obtained in Norway? Two answers might be urged to this question.

First, that the virus was not in reality that of the infecting sore at all, although obtained from the seat of an indurated chancre.

Secondly, that in other cases the suppurative action superinduced upon an induration by causes of irritation gives rise to a secretion possessing a morbid potency—an irritating contagious property—belonging to, and the product of, this artificially induced action, as distinguished from that belonging to and set up by the original syphilitic virus. In illustration of these positions I shall endeavour, as far as possible, to avoid all the theoretical explanations of the phenomena described.

In practice we occasionally meet with examples of infecting sores which are non-conformable to the description given of the typical indurated sore, such as the following:—

1. An initial lesion consisting of a vesicle, which becomes a pustule, then an ulcer like a soft ulcer, but which acquires a specific induration at some stage of its progress. Or,

2. A patient presents himself with one, or perhaps many, small ulcers possessing the characters of the local soft sore for some time, but before cicatrization a hardness developes itself at the seat of one of these chancres, the ulcerative action and secretion of pus continuing.

A close inquiry as to the exact date of the appearance of these forms of venereal lesion will generally bring one very suggestive fact to light—namely, that the first symptoms in these cases follow very shortly after exposure to contagion; i.e., within a few days. Never, within my own knowledge, has the interval been long between the date of the last sexual intercourse and the first appearance of the lesion.

The exact pathology of these chancres has yet to be worked out to the satisfaction of the medical world. One set of observers regard them as the strongest evidence of the identity of the soft and hard sore; another set think they see the characters of two different morbid processes evolved in the same seat, and regard them as the exponent and proof of a twofold inoculation—two seeds sown at the same place and time. With this we have at present nothing to do. The practical, the important fact in their history, to my mind, is this: that we can produce a lineal series of inoculations from these lesions, notwithstanding they are the precursors of general infection, and the effects of the inoculations are yet strictly analogous to those induced by the virus of the soft chancre. A reference to the contribution of Dr. Marston in the forty-fifth volume of the "Transactions of the Medico-Chirurgical Society" will supply observations corroborative of this statement.

The lesion which begins as a pimple; which becomes an induration; which is non-ulcerative in character for some time, but covered with a gummy sero-epithelial secretion; which takes on a molecular disintegration at the surface; which remains an indolent induration, until that induration is dissipated by a process of absorption, instead of being disintegrated by one of ulceration,—is the most common, the most typical form of infecting chancre; and the secretions from it are *not auto-inoculable*, unless we first of all induce suppuration on its surface. And this leads me to the second method of solving the practical difficulty.

Inoculation, in its more extended sense, implies the production of a disease in a healthy person, or in healthy parts, by the insertion or contact of some morbid secretions; and the effects may be simple or specific in character.

An eczematous eruption may be occasionally induced on the healthy skin by the contact of fluids from a neighbouring eruption; the secretion from a wound on an extremity may poison, inoculate, the lymphatics—by intravasation of a pus-globule, for instance, within a lymphatic tube,—and may produce an abscess at the nearest gland, with or without pustulation of the skin, corresponding to the seat of the obstructed lymphatic; the contact of pus from a balanitis or urethritis with another mucous membrane may give rise to pustular disease; and this mere insertion of ordinary pus into the integument with the point of a lancet will sometimes be followed by an inflammatory reaction. But the experiments of Dr. Van Roosbroeck, Dr. Piringer, and Sir Patrick M'Gregor, prove that a conjunctivitis, of different degrees of intensity, may be induced by the inoculation of purulent fluids obtained from various sources. Mr. Simon, in his essay on Inflammation (Holmes's "System of Surgery," vol. i.), remarks—"There is ample room to question the popular impression that only specific inflammations are communicable, and much reason for suspecting it, on the contrary, to be a generic and essential property of inflammation, that its actions (or some of them) are always, in their kind, to some extent contagious."

It is, in part, generally recognised by our profession that many diseases, such as skin affections and inflammations of mucous membranes, may develope contagious properties by undergoing changes of a dynamical character—that is to say, by affording purulent discharges.

There is, however, a great difference between these examples and those truly specific effects obtained by the inoculation of variola, vaccinia, glanders, and syphilis.

If we inoculate a healthy person with the fluids obtained from an indurated chancre or a constitutional syphilitic lesion, the product is an enlarged papule, induration, or hard chancre at the point inoculated, if the result be an affirmative one at all; and this effect is not developed in two or three days, but only commences after an interval more or less considerable. No one, as far as I know, has ever succeeded in propagating from the pus of an indurated chancre another, much less a series of indurations, in an individual labouring under constitutional manifestations of syphilis. The inoculations, if successful, are developed as vesicles and pustules about the third day, and the ulceration, if any, is not surrounded by induration. Yet this hardness is unquestionably the best, the only reliable or positive anatomical sign of the infecting character of the lesion. The individual having contracted the disease in the natural way, and once exhibited on his person the anatomical sign of the specific action of the virus, cannot be again subjected to it.

The evidence obtained, therefore, from the results arising from the auto-inoculation of pus from a hard sore *affords no conclusive proof whatever that those results depend directly upon the syphilitic virus which caused the original indurated chancre.* But it has been urged, that when you can obtain a lineal series,—a successive generation of pustules and sores,—it proves the existence of some special agent in the secretion inoculated over and above any of those simple effects to which ordinary pus might give rise. And unquestionably it is so, for we hold this very property inherent in the pus of the local soft sore as the proof of its being the vehicle of a specific virus.

With those varieties of the hard sore which I first mentioned, the results obtained by auto-inoculation are specific; but then these results are such as you get from using the virus of the local variety of sore, and there appears to be nothing illogical in ascribing them to that virus—in fact, to the virus of the local ulcer superimposed upon the infecting form of the disease.

I have applied the pus from a soft sore upon the surface of an indurated one, and have succeeded in producing an ulceration upon that induration exactly such as would have been obtained elsewhere, and the ulcer was certainly exactly parallel to those non-conformable chancres now described. Any matter thus put on an indurated sore, or mixed with the secretion of that sore, and again inoculated, would produce the same effect as if inoculated by itself in a healthy part of the body. Its nature is not changed by its being put on a hard sore and taken off again.

Further investigations into the dynamical change by which an inoculable secretion is developed are doubtless required to throw light upon the obscurity which yet hangs about it; but I am tempted to introduce some observations from Bidenkap which appear to bear upon the subject. This author, after showing that an indurated sore may afford an inoculable pus after the application of irritants, gives some illustrations of the effects of inoculating such artificially-produced pus in healthy persons. Three girls in his hospital inoculated themselves with matter obtained from the artificial sore produced on infecting ones. The first patient had a history of constitutional syphilis, and the inoculation produced in her a pustule without leaving an ulcer. The second had never had syphilis. She produced eighteen ulcers by her own act; and Bidenkap produced twelve more by re-inoculation from them. No symptoms of constitutional syphilis followed in her case during the six months that she remained under observation. The third was suffering from gonorrhœa, but had never had syphilis.

She produced a large ulcer by inoculation. No constitutional symptoms followed, but a year and a half afterwards she contracted in the natural way an infecting chancre, and subsequently had constitutional syphilis. The description given of these cases undoubtedly brings them under the category of local soft sores.

If the syphilitic poison itself was the cause of these effects, then, that virus gave rise to this local soft variety of chancre without being preceded or accompanied by any solid elevation of the skin, a circumstance in itself extremely unusual; but if we suppose that the effects might have been due to the *irritative quality of the matter inoculated*, which quality might have been acquired through that very artificial process of ulcerative inflammation by which the purulent secretion from the infecting chancre was produced. then these exceptional results may be referred to another action than that of the syphilitic poison.

In some cases, the two actions from the same inoculation may be distinctly and separately traced. In an experiment cited by Vidal, for instance, the secretion from a syphilitic pustule was inserted into a gentleman's hand and produced a pustule, which lasted fifteen days and then subsided. No further action took place until the thirty-fifth day, when the pustule broke out afresh and constitutional symptoms appeared, one hundred and twenty-eight days after the inoculation. In such a case the distinct actions must be allowed, whether they be both attributable to syphilis or not.

It is scarcely justifiable to repeat the dangerous experiment of inoculating another and healthy individual with fluid obtained from a pustule artificially induced upon a specific induration; but it has been done, and, what is more, it is positively asserted that the local affection only is produced by this plan, provided you are careful to take only the fluid from the pustule unmixed with any blood. The positive evidence, if substantiated, appears to be conclusive of the fact that you may not only have the products of a twofold inoculation at the same part, but that you may even succeed in propagating them separately.—*Lancet, March* 31, 1866, *p.* 336.

79.—ON THE TREATMENT OF SYPHILIS.

It has been asserted that the best mode of treating syphilis is by the repeated inoculation of the same poison which originally produced the disease. To demonstrate this fact was the object of Dr. Boeck's visit to England. The interest of the profession has in consequence been keenly excited; and it is due to them and to the public that the evidence afforded upon the subject should be fairly given.

1. It has been shown in my two former lectures that the inoculation of the syphilitic poison on a person who has real syphilis is not much more easy than the inoculation of the vaccine poison on a person who has been properly vaccinated. In all the experiments which I have performed, where due precaution has been taken, I have failed to produce a lineal series of inoculations from an uncomplicated indurated sore; and Dr. Boeck, during his residence in England, did not, I believe, succeed in producing such a result in any one instance. Mr. Walter Coulson has, however, had two cases in which, after repeated inoculation, he has obtained well-developed pustules. In one of these, which he was kind enough to allow me to see, I was informed that the patient was inoculated with the matter from a soft chancre before the attempts were made to inoculate him with the secretion of his own indurated sore, and that in some of these attempts at inoculation the same lancet was used to take the secretions from both kinds of sore. The lancet was wiped, but not washed after each attempted inoculation. Some of the secretion from the soft sore may therefore in this case have been left by the lancet on the hard sore, and thence removed so as to produce the supposed inoculation from the indurated chancre. Hence a possible source of error in this case; but in the second instance I could not detect any source of error. From the first of these cases Dr. Boeck continued to inoculate, and he also inoculated from some matter which he had received from Norway. This last-mentioned matter was unlike any that I have ever seen taken directly from an indurated sore. It was in considerable quantity, and presented all the appearance of pus derived from a well-formed abscess. If the matter had been collected from a large number of pustules, such as those produced in Mr. Coulson's cases, it would probably have presented the same appearance.

Now, whether matter of this description be a direct product of true syphilis, or whether it depend upon some dynamical change in the action of the part, it is essentially of a different nature, and formed by a different action, from any that naturally pertains to an infecting sore. This secretion, whether theoretically we suppose it to have its origin in syphilis or not, is practically not that which produces syphilis. In the change of action which has taken place during its production it has lost its sting, so far as its power of producing an indurated sore is concerned; and it has also lost, as far as our present evidence goes, its power of infecting a patient's constitution.

On the other hand, it has gained a contagious property which it had not before, and can be inoculated with a rapidity and certainty quite different from that which was possessed by the natural secretion of the part upon which it was elaborated. It is therefore changed, not only in appearance, but in vital power. It is different in its immediate effect and in its subsequent results. It, in fact, possesses all the characters of the secretion of a soft chancre, and none of those characteristic of an indurated sore. Whether therefore, I say, it be derived from an infecting sore originally or not, it must be regarded as resulting from an essentially different action, and as possessing qualities essentially different from the secretion of the infecting chancre. Practically, therefore, the secretion from a pustule and the secretion from an indurated chancre are the results of different affections, and it is evidently incorrect to talk of the one as of the other, although the two should have been produced in the same part, or originated on the same spot.

The very conflicting statement made about the auto-inoculability of the secretions of the indurated or infecting sore leave ample room for the suspension of our judgment, to say the least.

An Italian surgeon has quite recently put the subject of inoculation to the test of observation and experiment, and his results are totally opposed to those obtained by Drs. Boeck and Bidenkap.

Dr. Amilcare Ricordi ("Sull' Irreinoculabilita delle forme di Sifilide;" extracted from the "Annali Universali di Medicina," vol. clxxxv., Fasciculus of January, 1866,) has only this year published his paper. His object was to test the truth of the assertions of those syphilographers who declared they employed the purulent secretions from the indurated or true syphilitic sore for the purpose of syphilization, and to determine at the same time the truth, or otherwise, of an hypothesis put forth by M. Langlebert as to the different properties of the serous and purulent fluid of syphilitic lesions.

In syphilization if the indurated chancre, or mucous tubercle, does not yield pus, it is made to do so by slight artificial irritation, and the secretion is said to require new and peculiarly active properties, which adapt it for inoculation.

The experiment is an eminently simple one, and Dr. Ricordi instituted a series of observations with pus obtained from thirty-six infecting ulcers, and forty-three times with that obtained from ulcerating mucous patches. The fluids inoculated were manifestly purulent, he says; yet the inoculations were always followed by a negative result. Prof. Pelizzari, in 1865, he states, also made a similar series of experiments, which led to similar conclusions. Occasionally, although very rarely, an inflammatory reaction certainly ensued after the inoculation, but the morbid phenomena were never more than might fairly be ascribed to the traumatic effect of the instrument used, and the slight irritative action of the purulent matters inserted, for the artificial affection always disappeared spontaneously in a few days. How then, asked Dr. Ricordi, are we to reconcile the experience obtained at Christiania with the want of success at Milan and Florence?

I am further tempted to make a short and pertinent extract from Dr. Ricordi's pamphlet. Dr. Gustavo Bargioni communicated to Prof. Pelizzari the results of Bidenkap's experience in Paris. Dr. Bargioni personally learnt from F. Follin and his interne, M. Bouchard, that Bidenkap made between seventy and eighty experiments with the secretions of the indurated sore, but he never succeeded in producing a characteristic soft ulcer. The size of the lancet employed by Bidenkap, and his mode of using it, were open to some objection, as it appeared to MM. Follin and Bouchard, and they requested him to employ a needle such as is used for vaccination. This Bidenkap declined to do, although the interne had perfectly succeeded with it in inoculating the pus of a soft venereal ulcer.

A circumstance of some interest was the following:—There was a patient present suffering from a well-marked "mixed sore," so-called. M. Follin raised some objections to the pus of that ulcer being employed. Bidenkap, putting aside the objection, desired to employ the pus of this ulcer, and the result of the inoculation was such as M. Follin had predicted—viz., a characteristic soft ulcer.

Now to follow Dr. Ricordi tediously through his other observations, I will only remark that this observer succeeded in inoculating some of the animals with the pus of a soft venereal ulcer, but he failed entirely to engraft syphilis or any characteristic lesion upon them by inoculation with the virus of the hard chancre.

If an action be superinduced upon a sore which cannot be produced by any chemical or mechanical irritation, it shows that some fresh morbid process has been set in operation, and it would be manifestly unjust to attribute any results produced by inoculation from such a source to the influence of the secretion of the original sore alone. To assume this would be to admit a source of error which would run through the whole series of experiments performed.

Where a source of fallacy may be thus seen to lie at the origin of every series of inoculations, which unless guarded against may readily run through the whole, it is of the highest importance, in a scientific point of view, that the greatest precaution should be used in performing the first inoculation of the series, and especially that a fresh lancet, or a lancet that had been placed in boiling water, should be used. In the practice of Drs. Boeck and Bidenkap this precaution is not usually taken. When in a field of wheat we see here and there some oats, however much we may be assured that wheat only was sown, yet we feel satisfied that, either by accident or by seeds left from a former crop, or in some way or other, each blade of oats had its origin in a seed of the same nature. If allowed to grow together, a mixed seed would be produced. If the seeds from this were again and again sown in a soil in which the wheat would not grow, we might in time get a prolific crop of oats, but we could never suppose that the oats had originally been derived from the wheat.

That in syphilitic inoculation we have to do with at least two distinct actions may be proved, not only by experiment, but also by practical observation.

[It is urged in favour of this plan of treatment that it prevents the internal organs of the body, and the bones, from becoming affected with syphilis, and the relapses under such treatment are extremely rare. On the other hand, Mr. Lee states that he has never seen any severe complication of the deeper structures where the calomel bath has been properly used for primary syphilis.]

If we have reason to be disappointed with the evidence on the subject of the so-called syphilization here, we have, on the other hand, extensive and most accurately kept tables in Dr. Boeck's great work, which furnish abundant proof that the symptoms of constitutional syphilis will disappear under the continued irritation kept up by repeated inoculation. It is urged in favour of this plan of treatment that it prevents the internal organs of the body, and the bones, from becoming affected with syphilis; and that the relapses after such treatment are comparatively rare. We are not, perhaps in a position to judge of what means are at Professor Boeck's disposal in order to ascertain the number of relapses which take place. But in this country it would certainly be no indication of the number of patients permanently cured if we were to calculate the number who voluntarily returned after a four or six months' course of what must appear to them an ineffectual treatment. With regard to absence, under this plan of treatment, of diseases of internal parts, I can readily believe that the continued irritation on the skin would have the effect of preventing the morbid manifestations elsewhere; for I have observed the same fact with regard to the use of the calomel vapour bath in primary syphilis. The continued action that this maintains on the skin has apparently the effect of preventing the deeper structures from becoming affected; at all events, I may state very positively that out of a large number of cases treated both in hospital and private practice I have seen no severe complication of the deeper structures where the calomel bath has been properly used for primary syphilis. We have no means of instituting a comparison with regard to the efficacy of the treatment by repeated inoculation, and by the calomel vapour bath, respectively, with regard to primary syphilitic sores; because the former plan does not appear to have been tried before the evidence already existed of the patient's constitution having become affected; and if it had been tried, the conclusion, as already explained, would have been most unsatisfactory so long as the distinction between the infecting and the non-infecting form of the disease was not kept in view.

In secondary syphilitic affections we have, however, the means of instituting a comparison between these two methods of treatment. I have carefully gone over ten of the case-books kept at the Lock Hospital during the period that I held the office of surgeon to that institution, at a time when the calomel bath was frequently used. Excluding the cases which left the hospital before they were considered to be cured, on the one hand; and cases complicated by other diseases, not venereal, or which required some operation to be performed, on the other; these ten case-books gave the following results:—

Seventy-eight men were treated for constitutional syphilis by the calomel vapour bath, and discharged as cured during 426 weeks, giving an average of a little more than five weeks and three days for each case.

Eighty-seven women were treated for constitutional syphilis by the calomel vapour bath, and discharged as cured, during 605 weeks, giving for each case an average of something less than seven weeks. Many of these women were received into the asylum and remained under observation for different periods, not exceeding two years. The reason why the period of treatment was longer in women than in men was, that the proportion of cachectic cases in women (many of whom were received in a state of great destitution) was larger than in men.

It is not easy to get patients in this class of life to remain in hospital for primary affections, especially when attended with little local inconvenience, and the number of those who were admitted, and remained until properly discharged, for primary infecting sores, was therefore small.

I find that sixteen men were thus treated by the calomel vapour bath, and dismissed as cured, during a period of eighty-nine weeks, giving an average for each case of five weeks and four days.

Thirty-two women were similarly treated during 158 weeks, giving an average for each case of something less than five weeks.

Taking all the figures together, we have 213 cases treated during 1278 weeks, giving an average for each case of exactly six weeks.

If we compare with these figures the length of time employed in the process of repeated inoculation, we find 252 patients mentioned by Dr. Boeck ("Recherches sur la Syphilis," p. 471) as having been treated during 33,828 days, which gives for each case an average of rather more than nineteen weeks. The result which Professor Boeck gives with regard to the time required for the treatment of this disease with antimonial plasters by "derivation" is still more unfavourable. Small antimonial plasters, in these cases, were placed on different parts of the body, so as to keep up a continued series of small ulcerations. Eighty-five patients were treated for constitutional syphilis in this way, and, on an average, the treatment of each patient occupied a period of twenty-six weeks.

Professor Boeck mentions (p. 463) 537 patients as having been treated by calomel internally administered, each case having occupied, on an average, rather more than eighteen weeks and a half. He also mentions 3200 altogether as having been treated by different proportions of mercury (p. 470), each case, on an average, having required very nearly eighteen weeks. Out of these 3200 cases 108 deaths occurred.

Dr. Boeck gives some elaborate tables, showing the results of other modes of treating syphilis. The treatment by purgatives and iodide of potash are considered. It would prolong this lecture beyond its necessary limits to consider the conclusions at which Professor Boeck's figures point. Suffice it to say that the results obtained are not favourable with regard to any of the plans enumerated, and in none does the time occupied in the treatment appear to have been anything like so satisfactory as that which the statistics above given show to be required for the treatment of the disease by the calomel vapour bath.

The calomel vapour bath has, moreover, the advantage of being applicable where other means of treatment, including mercury internally administered, have been used, and failed. It may be given where the strength of the patient has been very greatly reduced, provided there be no organic disease of internal organs.

I must now say something about the mode of the administration of the calomel vapour bath, especially with regard to one or two points which I have reason to believe have as yet been very imperfectly considered by our profession.

The object of depositing the vapour of calomel on the skin is, first, to excite an action there, and, secondly, that something may be absorbed from the powder thus deposited. In order that such absorption may take place, it is necessary that the powder should be allowed to remain on the skin. It is also necessary that the skin should be soft and clean. If the skin be covered with dry scales, or if any grease be allowed to remain on it, it is obvious that its absorbing power must be greatly interfered with. In order to ensure the proper action of the skin, I generally recommend that a patient should take a hot-air or vapour bath first. When a free perspiration has been produced in this way, the skin is wiped dry, and a clean surface presented for the action of the calomel. Without this precaution, many baths may sometimes be taken without producing any sensible effect. On the other hand, when the skin is thus prepared, its absorbing power is often at once perceptible. A patient of mine, who has for years kept his skin in the most perfect condition by means of the hot-air bath, always shows the characteristic influence of mercury on the gums if he takes a calomel bath (from a scruple to half a drachm) two days in succession. This patient always inhaled as little of the vapour as possible. I may here mention that the action of the bath is materially assisted, in the majority of cases, by the inhalation of a certain quantity of the vapour; but this is by no means a substitute for the action of the calomel on the skin.

Messrs. Pollard, surgeons, of Alfred-place, Brompton, have now for a long time very efficiently carried out my idea of the way in which the calomel bath should be given. They have had some rooms fitted up, in which a patient may take a hot-air bath, or the first part of a Turkish bath; and when the skin is acting freely, the patient is rubbed down and placed in the calomel bath. I might give many cases where patients have recovered in a very short time under this mode of treatment. It is peculiarly applicable to cases in which the skin is thickened and scaly, either naturally or from disease. Two or three years ago, a patient was sent to me by my friend Dr. Marston, of the Royal Artillery. He had been under Dr. Marston's care, and I need not say that he had thus as good a chance of recovery as he could possibly have had from the administration of ordinary remedies. The body and limbs of this patient were covered with one mass of thick scales. Occurring in a young man, it was the most marked case of inveterate psoriasis which I have ever seen. He could not move without pain, and was quite incapacitated from doing his duty. This patient was requested to go to Messrs. Pollard's establishment, and to take daily a sweating bath first, and a calomel bath afterwards. In about three months I had the satisfaction of seeing this patient's skin quite clean, and his general health restored. He has since married, and has suffered no further inconvenience from his former most troublesome complaint.

In concluding the comparison between the treatment of syphilis by the calomel vapour bath and by syphilization, I would call attention to what appears to me to be a very important aspect of the matter—viz., the *practicability* of syphilization as a mode of treatment in this country. The method unquestionably makes great demands upon the time and patience of the sufferer and the surgeon, but particularly upon the former. We may obtain a rough approximation as to the time consumed perhaps by fixing three months as the minimum and eight as the maximum period over which the inoculations will extend. These inoculations are sometimes "terribly painful." During the whole period the patient must attend or be seen every third day for the purpose of being inoculated. The surface of his body will be the seat of numerous small ulcers and pustules, the marks of which, be they small or great, he will continue to bear upon his person afterwards. The majority of cases of syphilis, as witnessed in persons of average health and constitution, certainly recover in time under mercurial treatment without ever presenting that formidable train of symptoms which have very unjustly been ascribed to its use. Even if we should admit that mercury and other remedies only remove the manifestations, leaving the patient subject to an outbreak of syphilis at subsequent or remote dates, and that syphilization is not liable to this objection, still will not the sufferers, under ordinary circumstances, be more likely to prefer incurring the risk of subsequent relapses, or that of some remoter internal lesion, to undergoing a process of cure which will consume so much of their time, demand so much of their perseverance, and leave such traces—indelible they may be—of its action?

Again, the practice can hardly be capable of general application so long as the number of Lock hospitals and the sources for the supply of the chancre-matter is limited. So long as the inhabitants of country towns or villages may contract the disease, just so long will the pressing necessities of business, family, poverty, and other causes require that syphilitic patients shall be treated at their places of residence. In order to carry out syphilization effectually, not only will an abundant supply of matter be required, but supplies from fresh sources will be needed likewise in order to attain the requisite immunity, or, it may be, to effect the cure. Within the walls of Lock and other large hospitals and in those of our public services there will, of course, be no such difficulties as must be felt elsewhere. But in private the treatment by syphilization would tend to bring these diseases more and more under the care of specialists. Let the power, the superior efficiency, of syphilization be never so firmly established, there are difficulties attending its application which it behoves us to consider as men of the world, as members of a practical money-getting nation, where men are so extremely jealous of their time.

Jenner warned the profession in his day against the belief that every successful inoculation with matter taken from a vaccine vesicle was necessarily the real vaccine disease; and he especially notices that after a vesicle had suppurated it was very liable, upon re-inoculation, to produce an affection which was not followed by the legitimate results of the vaccine disease upon the patient's constitution. To apply the warning which Jenner gave to our present subject may not be out of place. It no more follows that the successful inoculation of matter from the surface of an indurated sore produces real syphilis than it follows that the production of a pustule by inoculation from what was once a vaccine vesicle necessarily communicates the genuine vaccine disease.

The transmission of a poison, and of an action produced on a poisoned part, may be clearly distinct; and to conceive them to be necessarily the same would be to give up a distinction which Jenner clearly and particularly recognised.

We conclude—

1st. That no evidence has hitherto been adduced satisfactory to the profession, that the infecting form of syphilis can be inoculated upon a patient who is at the time the subject of constitutional syphilis.

2nd. That both from a soft sore, and also occasionally from the surface of an indurated sore, matter may be taken which may be made to produce a number of local specific ulcerations having the characters of the soft chancre.

3rd. That during the continued irritation of such ulcerations the manifestations of secondary syphilis will disappear.

4th. That the time required for the treatment of syphilis in this way is so long, and the inconveniences attending it are so great, that it is not in any degree likely to be adopted in private practice in England.—*Lancet, April* 14, 1866, *p.* 391.

80.—ON SYPHILIZATION.

By James Lane, Esq.

[The following paper is an abstract of some observations made by Mr. Lane before the Medical Society of London, on the cases which have been under treatment by Prof. Boeck in the Lock Hospital. Dr. Boeck considers that by syphilization properly carried out constitutional syphilis can be completely and permanently cured, and that relapses, which are unfortunately so frequent after mercurial and other treatment do not occur at all, or in so slight a degree as to be of little or no importance.]

Twenty-one cases had been under treatment by Dr. Boeck during the last four months. Sixteen of these had been well-marked examples of secondary syphilis, uninterfered with by any previous mercurial treatment. Four had been cases of secondary disease previously treated by mercury, and one was a very severe tertiary case. Dr. Boeck had been desirous of confining himself as much as possible to non-mercurialized patients, as affording the best illustration of the value of the treatment, having found that a previous mercurial course interfered materially with the regular progress of the inoculations, and with the permanence of the result.

Mr. Lane then gave a brief account of one of the cases which had been longest under treatment. The patient was a girl, aged 18, with mucous tubercles and a well-developed squamous eruption. She was admitted into the Lock Hospital in August last, having undergone no previous treatment whatever. The inoculations were commenced by Dr. Boeck on the 5th of September with matter from a non-indurated sore, two punctures being made on each side of the chest. The inoculations were repeated sixteen times in this region of the body at intervals of three days, the matter being taken each time from the inoculations immediately preceding. The resulting pustules became smaller by degrees, and at last failed altogether. A like process was then (Oct. 26th) commenced upon the arms with fresh matter, and proceeded with there till Nov. 19th, when the inoculations failed. The same thing was then done on the thighs, and has been persevered with there till the present time (Dec. 18th); fresh matter, however, having been several times required. Punctures have also been made several times on the arms with matter from the thighs, with positive results. The treatment of this case might be considered as nearly but not quite concluded, it having extended over a period of three months and thirteen days. She is now evidently losing the susceptibility to receive the poison by inoculation, and latterly, as that susceptibility has diminished, her symptoms have been rapidly disappearing.

A second case was alluded to, the treatment of which Dr. Boeck considered to be now concluded. In this case all the original symptoms had disappeared, and the immunity to inoculation with fresh matter was nearly if not quite complete. The important question of relapse of course remains to be decided.

Mr. Lane stated that in none of the cases had there been any spreading ulceration at the seat of the inoculations. The earlier sores seldom exceeded half an inch in diameter, the majority, especially the later ones, having been much smaller than this. Their average duration had appeared to be about three weeks. The health of the patients had, in most instances, improved under the treatment; in none had there been any deterioration. The disappearance of the original symptoms, it seemed, could not be expected till towards the end of the treatment. On the contrary, a fresh accession of symptoms had been several times observed, and, in one patient, a severe attack of iritis. The inoculations, however, were persevered with notwithstanding. The patient with iritis had recovered in about the usual time without any permanent damage to the eye.

After alluding to some further details, Mr. Lane said there was one point of great interest which these experiments had served to illustrate. The inoculations had been practised with matter derived from indurated as well as from non-indurated sores. Matter from patients undergoing syphilization, but originally derived from an indurated sore, had been sent from Norway by Dr. Bidenkap, and had been inoculated on several of the patients in the Lock Hospital, with no appreciable difference in the size or appearance of the resulting pustules. Matter derived from a well-marked indurated sore in an out-patient of Mr. Walter Coulson had also been inoculated on one of the female patients, and well-developed pustules had resulted, from which many reinoculations had been made. The conclusion to be drawn from this fact was obviously adverse to the theory that had been put forward of late years—that matter from an indurated sore could not be inoculated upon another individual already the subject of syphilis.

In conclusion, Mr. Lane said that, although he and his colleagues had not yet sufficient evidence to enable them to form an opinion as to the curative power of syphilization, they had seen quite enough to induce them to consider the investigation of great interest and importance, and fully intended to pursue it as suitable cases came before them. His association with Dr. Boeck had inspired him with a profound respect for that gentleman's scientific attainments, as well as for his straightforward and truthful character; and he felt certain that in leaving England he would carry with him the good wishes and the sincere esteem of all with whom he had come in contact.

[Mr. C. Drysdale, who had watched the progress of the cases under Professor Boeck's care, observed that no damage appeared to be done by the inoculations, no phagedena had occurred, and the pain was almost nil.

Mr. De Meric considered that the advocates of syphilization were mistaken, and that the practice must be condemned whether it be considered in an experimental, a therapeutical, or a moral point of view.]

As an experiment, the successive inoculations of the matter of chancre, from various sources and from various species, were extremely interesting; the pathologist could only gain by observing the transformations, and might perhaps be led to the discovery of some great pathological principle. But the patients thus operated on would not gain by these proceedings; the possible results just pointed out were too dearly bought; and he (Mr. de Méric) thought—though he had himself inoculated on a comparatively small scale—that we were not justified in indulging in hundredfold inoculations upon the same subject. In a therapeutical point of view, he considered that syphilization was quite nugatory. Dualists would at once condemn it: because they would justly answer that inoculating the matter of soft chancres had had no effect on the disease diffused through the whole organism; and that, even where the operators succeeded, by dint of irritation, in inoculating the matter of hard chancres, the results were simply the phenomena of the soft sore. But even unicists might say that a patient suffered from generalized syphilis by his own peculiar receptiveness; and how could those who practise syphilization hope to act upon this peculiarity by inoculating a number of chancres? Unicists knew, in spite of their belief in one virus, how difficult it was to inoculate a hard chancre upon a syphilitic patient; therefore, seeing the ease with which soft sores could be multiplied, they must admit the inability of the latter to act upon the disease. When they saw the secondary eruptions fading during the successive inoculations, they could not help recollecting that such eruptions might also fade without any treatment whatever, as shown by the non-mercurialists. Therapeutically, the patients were therefore no gainers, and were tortured to no purpose..—*Lancet, Jan.* 13, 1866, *p.* 37.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

81.—ON PUERPERAL FEVER.

By Dr. Snow Beck.

Dr. Snow Beck lately read a paper before the Obstetrical Society of London on "Puerperal Fever," carefully detailing the particulars of two cases in which the uterine sinuses were shown to be pervious after death, and from which the following deductions were drawn :

1. The phenomena of puerperal fever may be produced by the introduction of poisonous fluids into the general system.

2. The uterine sinuses remaining pervious to the flow of fluids would afford a means by which the poisonous fluid or fluids would enter the system.

3. The pervious condition of the sinuses remained in consequence of the absence of that firm and persistent contraction of the uterus after childbirth which appears necessary to effectually close these canals, and prevent all circulation of fluid along them.

4. The secretion from the interior of the uterus was probably sufficient, when mixed with the blood, to induce the effects observed. And it would further follow that—

5. The various phenomena observed in puerperal fever may arise from this cause, modified infinitely by many incidental states ; and the various inflammatory actions and products observed in the course of the disease would not be the essential parts of the disease, but morbid phenomena which occurred during the course of it.

6. The primary, though not the only, object in the prevention of these attacks of puerperal fever will then be to procure a firm, complete, and persistent contraction of the uterus after the birth of the child, and thus effectually to shut off all circulation within the vessels of this organ.

The author considered that the cases gave a decided negative to the opinions that puerperal fever was caused by uterine phlebitis, lymphangitis, endometritis, metritis, or any similar inflammatory condition of the uterine organs, as no product of inflammation was anywhere discovered after a careful and even microscopic examination. The results were also opposed to the opinions of Cruveilhier, who compared the internal surface of the uterus to a vast solution of continuity, and the gaping orifices of the sinuses to the open-mouthed vessels of an amputated limb. For the internal surface of the organ was everywhere covered by a soft membrane containing all the elements of the mucous membrane and covered by a red mucous secretion ; whilst the gaping sinuses could only be compared to the open-mouthed vessels of an amputated limb when the uterine sinuses were pervious and admitted fluids to circulate within them,—the point of comparison not being the open orifices, but the open canals leading from these to the veins of the general system. The comparison between puerperal fever and surgical fever was also examined and considered imperfect, inasmuch as a natural condition of the vessels of an amputated limb was compared with an unnatural state of the vessels of the uterus, which ought not to be, and might in the majority of cases be prevented. The propositions given by Dr. R. Ferguson, in his valuable essay on puerperal fever, were examined. The author differed essentially from Dr. Ferguson on the causes of the vitiation of the blood and on the varieties of puerperal fever.

The effect of this noxious impregnation of the general system was next considered, and it was shown that the quantity modified the result in a most singular way—a small quantity being eliminated by intestinal or urinary secretion, whilst a larger dose killed. When the uterus was very lax, and admitted of a ready flow of noxious fluid through the sinuses, the woman was striken down as if by some fell pestilence, and sank in a few hours—" where the secretions are all suspended, and the patient sinks with rapidity." Where the deleterious fluid was introduced in smaller quantity, the system, after a vain struggle with the poisonous infection, sank in a few days, the chief morbid appearance after death being extensive peritonitis of a peculiar character, copious exudation of soft friable lymph, and much serosity. To a yet smaller amount of deleterious impregnation were attributed uterine phlebitis, metro-peritonitis, distension of the lymphatics with purulent fluid ; the effects being more local, and the inflammations being the consequences of the first changes induced. Whilst a still smaller amount of infection would produce low febrile conditions, extending over an indefinite period, and sometimes inducing purulent infiltration in various organs of the body. Each of these states being influenced by a variety of concomitant circumstances—as the original constitution, state of health, the character of fluids secreted, the existence of any diathetic disease, epidemic influences, &c.

On the question of contagion and the occurrence of epidemics, the author showed that the most experienced in this disease varied so much that no decided conclusion could be drawn. The prevention of the disease, it was urged, could be most effectually accomplished by procuring a complete and persistent contraction of the uterus after the completion of the labour ; and that the means usually recommended were not sufficient for this object, it being generally considered sufficient to procure such an amount of contraction as to prevent any hemorrhage, though it was necessary to go beyond this point of contraction before the safety of the woman could be secured. A nourishing and supporting diet was further necessary to remove the physical fatigue and mental anxiety of the labour, to restore the health from the waste occasioned by the previous months of pregnancy, and to enable the woman to pass through the subsequent changes which had yet to take place ; much, however, depending upon the previous habits, state of health, and other incidental circumstances.

On the subject of treatment, when the disease was once developed, the principles were considered to be—(1) to prevent the further injurious impregnation of the system either by obstructing the further flow along the uterine sinuses, or by removing the noxious fluids from the interior of the uterus ; (2) by supporting the system during the struggle in which it was engaged, and by meeting any incidental complication which might present itself ; and (3) perhaps a further source of treatment was now afforded, which might enable us to counteract, to some extent at least, the deleterious impregnation which had already taken place. The first would be attained by procuring, if possible, the further contraction of the uterus, or by inducing the coagulation of the blood in the uterine sinuses. But the principal curative means appeared to rest upon the removal of all noxious fluids from the interior of the uterus, by cleansing it with a tepid solution of any sulphite, or, hyposulphite of soda each day or oftener ; and should any fluid gain entrance into the uterine sinuses, it would probably be more beneficial than otherwise. The means to support the system were too well known to require further notice ; whilst deleterious impregnation, which had already taken place, might be met by the internal administration of sulphite of magnesia or lime, in doses of one scruple to half a drachm every two or three hours.

Dr. Graily Hewitt stated that, having had considerable opportunities of witnessing the disease now under discussion, he would give some of the results of his own observations, although it was impossible in a few words to include a consideration of all parts of the subject. He had long entertained the idea that a very close connection subsisted between a loose relaxed condition of the uterus after delivery and the supervention of puerperal fever. He had followed a plan of treatment based on this idea, and had frequently insisted upon it in teaching. He considered that the author of the paper had offered anatomical proof of what had been a matter of surmise among many obstetric authorities of late years—namely that puerperal fever and allied disorders are not necessarily and inseparably connected with the existence of inflammatory changes in the tissues of the uterus and neighbouring parts. He believed, with the author, that the disease arose from the introduction of putrescent or decomposing material into the uterine sinuses, and thence into the general circulation. This was the cause in by far the majority of instances, but he believed that the poison was sometimes introduced by other channels. He could not regard puerperal fever as a disease *per se*, and he considered it would be difficult to distinguish anatomically and pathologically between ordinary puerperal fever and that condition produced by the contagion of scarlet fever or other fevers in the lying-in woman; for though the cause might be different, the effect was or might be the same. The essential part of puerperal fever is poisoning of the blood, and this may be effected in various ways. He had, as he had already stated, observed a very close connection to subsist between the relaxed uterus and puerperal fever; he had always found the uterus larger than it should be at the outset of the attack, this increase of size being accompanied with tenderness and other signs. In this respect, therefore, he quite agreed with the author of the paper. It would afford confirmation of the truth of this view of the subject to describe the treatment which he (Dr. Graily Hewitt) had for some time adopted in cases of puerperal fever and its results. Disregarding entirely and completely the old ideas as to inflammatory changes in the uterus, he was in the habit of applying the bandage very tightly over the uterus the moment the first symptoms appeared, and of administering internally a stimulant diet, including large doses of alcohol. Other remedies were used, but they were quite subordinate. Depleting and lowering medicines were wholly omitted. The quantity of alcohol given was sometimes at the rate of as much as two ounces of brandy every two hours. This treatment he had seen cut the disease short in the course of twelve hours. He had the greatest confidence in the efficacy of such treatment in bad cases of puerperal fever, having seen not a few apparently hopeless cases recover under it. In reference to the prophylaxis of the disease, these observations were interesting. The binder served a very important purpose, and he was in the habit of paying the greatest attention to its careful application, believing that in a well-contracted uterus we have the best safeguard against puerperal fever. The facts related by the author of the paper bearing on this latter subject were very important.

Dr. BARNES said the subject was one of such paramount importance that it deserved a special discussion every year. He believed, if attention were thus continually brought to it, that much would be done in the way of preventing this the most destructive of all causes to puerperal women. It was impossible to advert now to more than one or two points. He agreed with Dr. Beck that puerperal fever was a distinct thing from typhoid or scarlatina, which often attacked puerperæ, but preserved their essential characters. On the leading idea of the paper—the necessity of contraction of the uterus as a preventive of puerperal fever—he would say, that contraction was eminently desirable to accomplish, but he had seen repeatedly puerperal fever occur in women after perfect contraction, and, on the other hand, no puerperal fever although the uterus remained relaxed. After the great discussion at the Académie de Médecine, Dr. Martineng published a memoir setting forth this view, and advocating the use of ergot and all means to ensure contraction as preservative against fever. Dr. Barnes having under observation 2000 or more cases yearly of poor women attended by the Royal Maternity Charity, thought he was able to negative the proposition that puerperal fever was more frequent amongst the poor and badly fed. The cases in the charity were very rare. For one case amongst these women, he saw ten amongst the easier classes. As a means of preventing the loss of blood—as hemorrhage undoubtedly predisposed to puerperal fever—he had found nothing of equal efficacy to the injection of a solution of perchloride of iron into the uterus after clearing out the cavity of placental remains and clots. He had used this plan for several years, and in a large number of cases after labour and abortion, and had always had reason to congratulate himself upon the result. The perchloride of iron had the further advantage of being antiseptic. He had certainly saved several women from death from flooding, and believed these and others had been rendered less liable to puerperal fever. In reply to a question from Dr. Timothy Pollock, the President stated that the plan he adopted was to carry in his "obstetric bag" a saturated solution of the salt. Of this solution he used one part to eight of water. But he thought that even a weaker solution was sufficient. It instantly coagulated the blood in the mouths of the uterine vessels.

Dr. SNOW BECK briefly replied, and stated that he considered puerperal fever essentially distinct and different from typhus fever, typhoid fever, scarlet fever, or any of the acute specific diseases; each of the latter being caused by a distinct and definite poison, which might be communicated from individual to individual in the same ward; whilst the former was produced by the introduction of putrid or similar fluids into the general system through the sinuses of the uterus, when they remained pervious after a confinement, and was *not* communicated from individual to individual in the same ward. Perhaps the term "puerperal fever" was not the most appropriate, but it had been consecrated by long usage, and it was not desirable to change it without good cause. There was a great distinction between puerperal fever and febrile states occurring during the puerperal period, the former being incurable up to the present time, the latter readily admitting of cure. This distinction had been clearly drawn by Professor P. Dubois. He had used the soluble sulphites as injections for cleansing the interior of the uterus, from the opinion that they were the most effectual agents in correcting the condition of the fluids secreted. But he had no doubt other agents would be found equally and perhaps more efficacious, and amongst them possibly the preparation of iron mentioned. But if it were even admitted that this plan of treatment was advisable, and devoid of the great danger sometimes attributed to it, then an important step had been attained. He, however, would strongly urge the necessity of procuring complete and persistent contraction of the uterus, and of carrying this contraction further than was usually considered sufficient, as being one of the most effectual means of preventing this most serious disease. He thought that by separating the many diseases occurring during the puerperal period, and included under the one designation "puerperal fever," though differing essentially in their nature, course, and treatment, great good would result.—*Obstetrical Transactions, Vol.* vii., 1866, *p.* 31.

82.—ON THE USE OF WIRE-RIBBON IN SOME CASES OF DIFFICULT TURNING.

By Dr. VAL. HEYERDAHL, Bergen.

The application of a running noose of tape round the ankle of the child, in order to facilitate version, is easily made at the outlet of the pelvis, without any special contrivance for its performance. But when the feet are in the brim or pelvic cavity, this little manœuvre may be attended with great difficulty, and must sometimes be dispensed with altogether (as impossible).

To remedy this difficulty, several more or less useful instruments have been constructed, and in some text-books of midwifery students are recommended always to carry a "proe-nœud" in their obstetrical armamentarium. I will only cite the names of some inventors, Pugh Neverman, Gerner, Braun, Trefurt van Huevel, Wasseige, &c., to show that their number is not so limited. Of modern writers upon the subject I will mention a most valuable treatise by Dr. Hyernaux, of Brussels. This distinguished accoucheur points out most impressively that, in some cases of difficult turning, an instrument to apply the tape is indispensable; at least, he has succeeded by its aid where others have completely failed in accomplishing version. He pretends that he, by his instrument for applying a running noose of tape high up in the vagina, has been able to turn in cases where embryotomy apparently was the only method practicable. Dr. Hyernaux had the kindness, during my stay in Brussels two years ago, to show me his instrument and explain the manner of using it. It is a staff of German silver, with a horse-shoe shaped bi-valvular tube, with open bevelled extremities fixed to its top. At the handle is a spring, by means of which the horse-shoe shaped tube can be opened into two semi-circular grooved valves. A loop of the running noose of tape is put into, and fastened in, the bi-valvular tube. The instrument is then introduced, with the horse-shoe, along the arm and hand of the accoucheur, up to the foot, which is then slipped into the circle formed by the tube and the free part of the loop, between the two extremities of the horse-shoe. By pressing on the spring the tube readily opens, and lets the loop of the running noose free, surrounding the child's ankle. It is then tightened by pulling on the free ends of the noose.

I found the instrument rather ingenious, and easily applicable, and, as far as I could judge without putting it to test, the very best of the many invented for the same purpose, by other accoucheurs, from the time of Smellie and Siegemundin up to the present day.

I agreed with Dr. Hyernaux in his views. I had myself sometimes found it exceedingly difficult to get a running noose of tape up to the foot, and in such an emergency much missed the aid of a proper instrument. I therefore procured one of Hyernaux's, with the view of trying it on the first opportunity.

But at the very first case of difficult version I met with on my return to Norway, I had forgotten to carry the instrument along with me, which necessitated the employment of a substitute for it, which I am now going to describe.

It was a case of transverse presentation, in the country, some miles from Bergen, in which the diagnosis had not been made out, and the labour, therefore, had gone on a considerable time unrelieved,—the waters having flowed away about two days before my arrival. The pains had been rather energetic for the last twelve hours, with strong bearing down. I found the left shoulder presenting low down in the vagina, and considerably swollen; the uterus and os firmly embracing the fœtus. The fœtal heart was heard indistinctly about the umbilicus. I put the woman—a strong primipara at the term of gestation—under the influence of chloroform, and proceeded to turn. It was impossible to push the bulk of my hand into the uterine cavity, although the bearing down pains were very much lessened ; but with two fingers within the os I reached and got hold of one foot, aiding this purchase by forcing the pelvic extremity of the fœtus lower down with the other hand. So far I succeeded ; but I soon found that I wanted strength enough in my finger to bring the foot farther down than to the brim, the foot being in such a degree slippery that I lost my purchase every time I tried to pull down. At the same time it was impossible to raise the presenting shoulder up to get a little more room. I tried in vain to bring up a tape round the ankle. I failed repeatedly on account of the looseness of the tape, moistened in the discharges from the vagina, and I regretted very much that I had not the instrument of Hyernaux at hand, while I was convinced of its usefulness and importance in this case.

It then struck me to try another material for a running noose, with more stiffness and not so liable to slacken in moisture as common tape. I found amongst a large stock of ribbons, tapes of different kinds, &c., a peculiar sort of tape with thin wires interwoven, commonly used by milliners to give bonnets their different shape and fashion, called wire-ribbon.

I took about a yard of wire-ribbon, half-an-inch broad, doubled it up, and formed it into a running noose. I made the loop a little elongated, and large enough to slip a foot through, and twisted the free ends a little to give them more stiffness. I now succeeded in sliding a well-oiled running noose of wire-ribbon up to the foot, and to get a firm hold of it, and then, by moderately pulling on the foot, securely grasped in this manner, and at the same time pushing the presenting shoulder upwards with the other hand introduced into the vagina, I was fortunate enough to turn it in no time. Soon after, the child was born by nature alone, with the exception of a little manual force to extract the head. The child was dead ; but the patient did well, according to the report given me two days thereafter.

This case is, I think, of some interest for the practitioner, because it proves that the difficulty of grasping the foot high up in the vagina, by a running noose of tape—sometimes baffling our best endeavours—may be overcome without instrument in so simple a manner,—namely, only by substituting the material commonly used—tape—by another equally simple material—wire-ribbon, to be procured wherever bonnets are worn.

Of course the method of applying wire-ribbon may be modified in different ways ; for instance, by doubling the wire-ribbon, then twisting it in its whole length, only the uppermost portion being left untwisted and forming a loop. To the other end is fixed a handle, made of a small piece of wood. This little apparatus is used in the same manner; the only difference is, that the loop is tightened by augmenting the number of twistings by turning the handle. The material possesses this quality, that the twistings once made, do not untwist again, and thus the foot is securely and firmly grasped.

But I am inclined to think that the method put into execution in my case is perhaps the simplest, and, therefore, also the best.

Should the stem of the running noose give too much during its introduction, this inconvenience may be remedied by using a small surgical forceps to introduce it with.

There is one drawback to its use; wire-ribbon is sharper and more apt to cut than common tape, and I was obliged to protect my hand by means of a handkerchief. Around the ankle of the child was left a deep furrow, but it had not cut through the skin.

By applying the wire-ribbon running noose to the feet of the dead body of the child, I was not able to cut through, although I purposely used much more force, and for longer time, than advisable in any circumstance connected with the operation of turning a living child.—*Edinburgh Med. Journal, Jan.* 1866, *p.* 673.

83.—A REMARKABLE CASE OF "MISSED LABOUR": THE FŒTUS BEING RETAINED IN THE ABDOMEN FORTY-THREE YEARS.

By R. W. Watkins, Esq., Towcester.

On January 10th, 1866, I visited, by request, Elizabeth Jones, aged 74, widow of William Jones, a small village shopkeeper at Stoke Bruerne, Northamptonshire. She gave me the following history, which I will narrate as nearly as possible in her own words.

About forty years ago, she was in labour with her second child. Her first, born two years previously, having had water in the head, had survived its birth only a few hours. The labour being very lingering, they had sent for my late father, who attended her, and remained with her during the night. On the following day, being very busy, and the labour having made no progress, he left her in charge of the midwife who was in attendance upon her. The pains were lingering, but not very severe. On the third day, she felt something "drop down suddenly inside her; and the child, the movements of which she had constantly felt up to that time, at once became cold as a stone." She was in great pain, and my father attended her for a considerable period. She refused to allow any other surgeon or physician to attend her, or to have any operation performed. She gradually improved in health, although she was very weak for a long time, and did not decrease in size for several years.

Upon examination, I found her much emaciated, with a hard bony tumour in the lower part of the abdomen, exactly resembling the fœtal head. It could be easily moved from side to side; and on careful manipulation, I believe that I felt the back part of the thorax in close approximation to it. She was evidently sinking from chronic renal disease; and, knowing that her end was approaching, had sent for me to request that I would make a *post mortem* examination. She had previously made a similar request to Mr. William Knott.

The story was corroborated by some of her neighbours, and by the rector of the parish, who had heard from my father a full report of the case. It was also stated, that at different times three little bones, "like finger bones," had come away from her; but they had not been preserved.

On referring to old ledgers, I found the entry of the attendance on October 8th, 1822, in the handwriting of my grandfather, with the subsequent payment of the fee marked in my father's handwriting, and with his initials.

The woman died on January 13th, and the examination was made on the 15th by Mr. William Knott, in the presence of Mr. Knott, sen., Mr. Garlike, and myself. On making an incision through the abdominal parietes and opening the peritoneum, we immediately observed a hard white substance, which proved to be the vertex of a fœtal skull; and, on enlarging the opening, a perfect fœtus was extracted without difficulty. It was covered with plastic lymph; the limbs were flexed anteriorly on the body, and the head bent forwards to meet them, in the manner usually depicted in plates of the gravid uterus. It was attached by the umbilical cord to a vascular tumour of about the size of half an orange, which appeared to be the atrophied placenta, and which was connected by ligamentous attachment to the peritoneal covering of the broad ligament near the left ovary. One portion of this vascular tumour appeared to be a mass of unorganised lymph, containing fluid. Neither the fœtus nor the supposed placenta had any adhesions to the peritoneum, except the ligamentous attachment I have already mentioned. The uterus was perfectly normal. There was no cicatrix or other marks of injury on any portion of its surface. The ovaries were pale, but quite natural in size and form. There were no adhesions of the peritoneum in any portion of the abdominal cavity, and no appearance of previous inflammatory action in any part of the large or small intestines. The kidneys were extensively gorged with venous blood, and very friable; the left being more affected than the right. The spleen also was much congested and friable. The stomach and liver were healthy. The gall-bladder contained about twenty hard dark gall-stones. The pancreas was very much diminished in size. In front of the aorta, and immediately above the inferior mesenteric artery, to which it was connected by condensed cellular tissue, was a white encysted tumour, of about the size of a hen's egg, containing a milky fluid.

The above extraordinary case is, so far as I am aware, quite unique, and will be interesting, not only as an instance of recovery from tubular gestation and probably rupture, but also from the comparatively slight local and constitutional effects of a foreign body retained in the cavity of the peritoneum for more than forty-three years. The specimens have been forwarded to Dr. Barnes, for exhibition at the meeting of the Obstetrical Society.—*British Medical Journal, March* 3, 1866, *p.* 223.

84.—ON ENLARGEMENTS OF THE UTERUS WHICH FOLLOW ABORTIONS, PREMATURE OR NATURAL CONFINEMENTS; WITH CASES.

By Dr. Snow Beck.

(Abstract of a paper read before the Obstetrical Society of London.)

The author remarked that these enlargements had been long recognised by pathological anatomists, and quoted some microscopical observations by himself, and communicated to the Medical Society of London in 1851, which showed that the pathological condition essentially consisted in an enlargement of the muscular tissue of the uterus, without the presence of any inflammatory or heterologous deposits. The causes were considered to depend chiefly upon—(1) a want of complete and persistent contraction of the uterus, which permitted an increased circulation of blood in the gravid organ, and interfered with the changes which took place after parturition; and (2) on the partially developed state of the uterine tissue in abortion, which appeared to be unfavourable to the development of those changes necessary to its complete reduction in size. The enlargement of itself gave rise to few and comparatively slight symptoms, unless it existed to such an extent as to be felt as a tumour in the hypogastrium; but it rendered the patient liable to profuse hemorrhages, coming on suddenly and without appreciable cause. These enlargements might exist for many months, or even for some years, without any symptoms of importance; but from the recurrence of the catamenia, or other causes, congestion of the enlarged organ was gradually induced;

or congestive inflammation, which may be either of the whole or of any portion of the uterus, might take place, the usual symptoms of uterine affection being then present. Amongst the subsequent changes which took place were anteversion and retroversion, with more or less bending of the organ, which lesions interfered with subsequent impregnation. But a more important change was a gradual hardening of the organ, which reduced it to an indolent state, and rendered it very rebellious to treatment. The modification of the symptoms thus produced was shown by the cases recorded, and the physical examination of the organ detailed. The author found that in these cases the uterus was equally enlarged, smooth, pyriform, the cavity enlarged, and the orifice open. The sound readily passed to an extent varying from three to five or six inches. When inflammation was added the organ became tender, the arteries were felt to beat with more or less force, and the interior became very sensitive. Subsequent and various alterations were made: the lips became enlarged, often lobulated, projecting into the vagina, red and raw in appearance, and bounded by a distinct line, which marked the division between the mucous membrane of the vagina and that of the uterus. It was this condition of the organ which had been so frequently described as ulceration, although no such morbid change actually existed. With respect to the treatment, various cases were recorded showing the importance of injecting the cavity of the uterus with astringent lotions, and the safety with which it could be done, provided the actions of the uterus were perfectly quiescent. The cavity of the uterus being enlarged, and the orifice open, impregnation readily took place; and the physiological changes which followed were decidedly the best means of restoring the organ to the healthy state. Congestion or inflammation, when present, would have to be met by the usual means; and when the uterus was in the hardened, modified condition, in addition to the ordinary means of treatment, it would require some local stimulant to rouse the local action, and enable the other remedies to act. For this purpose cauterizing the lips with potassa cum calce had been generally employed.

Dr. Barnes observed that he continued the practice of injecting a solution of perchloride of iron into the uterus to arrest hemorrhage after abortion and labour, and with excellent effect. He no longer dreaded flooding as of old. So far he could illustrate by experience the safety of intra-uterine injections. But he thought a more desirable method of applying fluid styptics or caustics to the inner surface of the uterus would be by swabbing; that is, soaking a bit of sponge or cotton-wool in the liquid, and passing it into the cavity. He had contrived an apparatus for this purpose. An excellent plan of applying solid nitrate of silver was one he had learned from Sir Benjamin Brodie. That eminent surgeon dipped a silver probe in fused nitrate of silver, thus obtaining a thin stratum, which could be passed freely and safely into a sinus. This was the safest way of cauterizing the inner surface of the cervix or body of the uterus.

Dr. GREENHALGH said that Sir J. Y. Simpson had described these enlargements under the terms of subinvolution or incomplete involution of the uterus. He (Dr. Greenhalgh) quite agreed with the author as to their frequency and the obscurity of the local symptoms. He regarded it as a common cause of sterility, but that where impregnation resulted it was always curative, except where abortion ensued. In this disease he had found the uterus enlarged, flabby, and ill-defined, the sound entering an enlarged cavity four inches, and even seven inches and a half. He considered that hemorrhage was not a frequent symptom in this disease, and, where it did exist, was mainly attributable to some affection of the lining membrane of the uterus. He advised, where hemorrhage was frequent or profuse, so as to affect the general powers and resist the ordinary treatment, the injection of the compound tincture of iodine into the uterus; but laid great stress upon the importance of first freely dilating the internal os uteri, which dilatation in itself was more or less curative. He had found resolvent and sedative pessaries of value, as also douches of tepid and cold water with a Kennedy's syringe, and medicated fluids. He likewise advocated the adminstration of tonics, with the iodide of potassium and liquor of the ergot of rye, and alterative doses of the bichloride of mercury. He relied greatly for success upon improvement of the general health, out of some impairment of which this affection frequently originated and persisted.—*Lancet, March* 17, 1866, *p.* 288.

85.—ON HEMORRHAGE FROM DETACHED PLACENTA NEAR THE EIGHTH MONTH OF GESTATION: LABOUR INDUCED BY BARNES'S DILATOR: RECOVERY.

By W. J. TUBBS, Esq., Upwell, Cambridgeshire.

Mrs. ——, aged 44, had had three children, each labour being normal. She said she was standing in a cart on the 25th of November, and giving her little girl to her husband, who had just got out at his own door, when the horse went on and she was thrown forwards, but was saved by her husband from falling. She was frightened, but soon recovered from the shock. During the night, she got out of bed to pass urine, when a large quantity of blood poured from her. When she returned into bed, the hemorrhage ceased. In the morning, she rose and continued her domestic duties as usual.

On that day week (December 2nd), she had a return of the hemorrhage, which according to her account, was profuse. It came on while she was in bed, and continued until she was faint. Her husband got up and came for me about twelve o'clock at night. I went immediately, and found her still faint. She had lost a great deal of blood, which was still oozing from the vagina. The os uteri was tilted upward and forwards; it was a little larger than a sixpence, and rigid. I plugged the vagina with a silk handkerchief; applied cloths dipped in cold water over the pubes and to the vulva; raised the legs; admitted air freely into the room; removed some of the bedclothes; and gave twenty drops of oil of turpentine and a dose of ergot as soon as I could.

I watched her the following day and night, giving her diffusible stimulants, with an egg in some tea or gruel occasionally. She did not have any pains from the very first of my seeing her.

I left word that, in the event of any more loss, I was to be sent for immediately; and that she was not to get up for some days. In the course of two days, she came down stairs at noon, and continued much the same until seven o'clock in the evening, when, on crossing the room, the hemorrhage returned, and she fainted. She was taken to bed, and I was again sent for, and arrived shortly afterwards, when I found her very pale and almost pulseless. The vagina was filled with coagulated blood; the os uteri flaccid, and a little more dilated than on my first examination. I could just make out that it was a vertex presentation. I did not think it advisable to rupture the membranes, as I might, if necessary, turn better with them entire. I decided to use No. 1 dilator, and deliver by turning as soon as the os became sufficiently expanded. Desiring assistance, I sent to Wisbeach for the first surgeon who might be in the way, and at once began dilating the os. I found some difficulty in making the India-rubber remain in the os; but at last I succeeded by passing it in with a piece of whalebone. This answered admirably. There was no pain complained of during the dilatation; and, when the os was sufficiently opened, I left off injecting, and allowed it to remain until the arrival of the surgeon. When Mr. Wm. Groom arrived, he examined the expanded os, and agreed with me that I might introduce my hand. Just at this time, it seemed that the dilator had got its full volume of cold water; for, in attempting to inject a little more, to show Mr. Groom how it acted, the dilator burst with a loud report. As soon as the gush of water had come away, I passed up my hand by the side of the head and brought down the feet. The only little difficulty I experienced was from the os uteri contracting on the head, which I liberated as soon as I could introduce my finger into the mouth. The patient had not felt the child move for some hours; and it was still-born.

Dec. 15th. She was going on favourably, feeling only weak from the excessive loss she had sustained.

I have now used the dilators three times, with good results.—*British Medical Journal, Jan.* 20, 1866, *p.* 67.

86.—ABSTRACT OF A PAPER ON PROCIDENTIA UTERI.

By Dr. J. Marion Sims, Honorary Fellow of the Obstetrical Society.

[Several separate and independent conditions must exist in a case of procidentia uteri. Thus, there must always be a broad pubic arch, with very divergent rami and a relaxed perineum; and then the axis of the uterus must be turned back in a line with that of the vagina and the pelvic outlet; in other words there must be a retroversion. With the uterus anteverted, a procidentia is utterly impossible, be the attendant circumstances what they may.]

Huguier has written extensively on procidentia uteri, and I believe he was the first to point out the distinctive characteristics of its anatomical peculiarities. He found elongation of the cervix in all cases, either above or below the insertion of the vagina; and he suggested and performed amputation of the neck of the uterus in every case, and with great success.

I amputate the cervix only when its lower segment is too large or too long, and projects so far into the vagina as to present a mechanical obstacle to the retention of the uterus *in situ* when replaced. If there should be elongation of the infra-vaginal cervix, amputation is the remedy; but we often find procidentia without any extraordinary elongation of the infra-vaginal portion of the cervix. There is then nothing to amputate.

In these cases Mr. Baker Brown, Dr. Savage, and others, contract the vulvar outlet by the perineal operation; but generally I prefer to narrow the vagina above, which usually very effectually retains the uterus in something like a normal position within the pelvis.

The idea of narrowing the vagina is by no means new. I suppose we may justly claim it for the great Marshall Hall. However, I do not think the operation ever succeeded till my own day, and this success is due wholly to metallic sutures.

The author then described a case in which he observed that the descent was not by the protrusion of the cervix uteri, but invariably by a prolapse of the anterior wall of the vagina, which always preceded the cervix, and drew down the uterus. He noticed also that, by pinching up the anterior wall of the vagina into a longitudinal fold, the parts had no tendency whatever to come down; and that it was impossible for the patient to force them down if the anterior wall of the vagina was prevented from descending. Hence the idea of removing the redundant portion of the anterior wall of the vagina; but "it did not occur to me to operate simply by removing strips of vaginal mucous membrane. I seriously proposed to this lady to make a complete vesico-vaginal fistula, by removing at one blow, as it were, a large portion of the base of the bladder with the anterior wall of the vagina. She agreed to it, and it was adopted."

Proposing to excise the anterior wall of the vagina, I hooked it up with a tenaculum, pulled it well towards the posterior wall, and then grasped the base of the mass thus elevated with a pair of curved forceps made for the purpose, acting on the principle of Ricord's phymosis forceps, which held the parts firmly embraced, while with scissors cutting under the forceps I removed, at once, a very large portion of the anterior wall of the vagina. The portion removed measured two inches and a half transversely, by two inches and five-eighths longitudinally, and was very thick. The chasm made by this operation was fearful, the lateral retraction of the divided edges being so great as to present at a superficial glance some difficulty in bringing them together by sutures. There was, however, no trouble whatever.

The bleeding was not profuse; but I at once rapidly filled the chasm with cotton, to stop the hemorrhage by pressure. A few minutes sufficed for this; and then the tampon was removed for the purpose of closing the edges of the opening by transverse sutures. My surprise was equalled only by my delight, when I found that I had not succeeded in doing what I intended; for instead of excising the base of the bladder with the anterior wall of the vagina, I had, by the tenaculum, simply raised the hypertrophied vaginal tissue up between the blades of the forceps, luckily separating it from the lining membrane of the bladder, which remain intact. Thus, by a mere accident, the operation was really far better than if I had succeeded in accomplishing what theoretically I proposed to do.

The lateral edges were brought together longitudinally by silver sutures passed transversely. She was soon well, and is so to this day. The operation was done nine years ago. This method of operating was continued till 1858, when an elderly woman, with an enormous procidentia of fifteen or twenty years' standing, was sent to the Woman's Hospital by Dr. Duane, of Schenectedy. It was a very bad case indeed. I operated by the plan of simple denudation over an immense surface; the parts were brought together and healed kindly; but I had not removed tissue enough, and there was a considerable cystocele left, which required that she should use a pessary; and then it was that I devised another method of operating. Instead of the broad scarification of the anterior wall of the vagina, I simply removed the mucous membrane in the form of a **V**, the apex being near the neck of the bladder, the two arms extending up on the sides of the cervix uteri. These two denuded surfaces were brought together by silver sutures passed transversely, thus making a longitudinal fold narrowing the vagina and crowding the cervix backwards. This simple operation was thus repeatedly performed, and always successfully, by Dr. Emmet and myself, at the Woman's Hospital, from 1858 to 1862, when I left New York.

In Paris I had occasion to perform it for Sir Joseph Olliffe on an old lady sixty-five years of age, who had had procidentia for twenty years. The parts united; the uterus was held in its place, and she returned home in a fortnight. Her general health was very feeble, in consequence of a long residence in India; and in two months the whole cicatrix gradually gave way, and the procidentia was reproduced. This was the first and only case of failure that I had ever seen after this method. The operation was subsequently repeated; but this time, instead of a **V**-shaped scarification, it was made in the form of a trowel, the point presenting below, the shoulders above in the anterior cul-de-sac The denuded surfaces were brought together by transverse silver sutures. A small portion of tissue was left undenuded, for the purpose of permitting the escape of any secretions naturally forming in the shut pouch.

It is not my intention to draw a parallel between this and the perineal operation for procidentia. I only wish to add another resource to our means of permanent cure in this distressing affection. So far then, as mere surgical resources are concerned, we have three processes from which to choose; always, of course, adapting this choice to the peculiar exigencies of the case.

1st. Amputation of the cervix, as recommended by Huguier, when its infra-vaginal portion is too long. I have often seen procidentia cured by this alone.

2nd. The perineal operation, as performed by Mr. Baker Brown, Dr. Savage, and others; and

3rd. The operation of narrowing the vagina by the trowel or triangular-shaped denudation on its anterior wall.

Mr. CHAMBERS, at the meeting of the Obstetrical Society at which the foregoing paper was read, said he felt that the Fellows of the Society were much indebted to Dr. Sims for his valuable contribution to female surgery; but he submitted that even this operation was inadequate to meet the requirements of all the cases of complete prolapsus uteri which come under the notice of the surgeon from time to time. Thus, this, which may now be called the anterior operation, only takes rank with that which may properly be called the posterior operation, or Mr. Baker Brown's. Mr. Brown has never asserted that his operation is successful in every case of prolapsus uteri. On the contrary, he has for years past combined the two operations in cases necessitating such a procedure, though he (Mr. Chambers) was bound to say that the anterior operation, as performed by Mr. Brown, was a very simple one when compared with Dr. Sims's. He had had two very bad cases of this kind under his care within the last twelve months. One case was of three years' standing. This lady had not been able to leave her house for more than two years. He performed Mr. Brown's operation in November, 1864, and the patient is now in excellent health, having had no signs of a relapse. The other case was that of a poor woman who had suffered from a complete form of the disease for "over two years." Mr. Brown's operation was performed in this case, and for six months she has discharged the duties of her station with perfect ease and comfort. This patient came to the out-patients' room a month ago, and the parts appeared as perfect as when the operation was first performed. There are two important reasons why the posterior operation so often fails in the hands of some operators—first, the operation itself is *in many cases* very imperfectly performed; and second, the patient is permitted to get up and walk about long before the united surfaces have had time to become perfectly consolidated, or the enlarged uterus to become reduced to anything like its normal size and weight. Cases of this kind ought not to be permitted to leave their bed for eight or ten weeks at least, if the cure is to be permanent.

The PRESIDENT said he had asked for the experience of Fellows on the result of Mr. B. Brown's perineal operation, because, if this were found to fail, the argument in favour of Dr. Sims's operation was much strengthened. He had himself performed Mr. Brown's operation several times. He had found prolapsus return in spite of the provision of a good perineum. Further, he had known complete prolapsus take place in virgins; in one case especially, that of a girl aged sixteen, prolapsus was caused by epileptic fits, the vulva and perineum being quite normal. Again, he had repeatedly seen the uterus remain *in situ* notwithstanding large perineal laceration. It was clear to him, therefore, that defective perineum was not the cause of prolapse, and that operations for restoring the perineum or contracting the vagina posteriorly could not be expected to cure it. It seemed to him that Mr. Brown's operation was performed in the wrong place. It had no effect in providing support where it was wanted, namely, anteriorly, at the connection of the cervix uteri with the base of the bladder. This object seemed to be perfectly accomplished by the admirably reasoned-out and ingenious operation of Dr. Sims. Dr. Barnes briefly described his view of the etiology of the hypertrophic elongation of the cervix. It began with congestion, inflammation, and partial eversion of the lower margin of the cervix. The outer or vaginal surface of the vaginal portion was the more fixed, whilst the inner aspect of the canal more easily bulged outwards. Then the products of congestion became organized in the tissues, the fluid elements being absorbed. This process continually going on—eversion or growing out of the rugæ of the cervical canal—extension and hypertrophy went on. A demonstration of this kind was obtained by passing the uterine sound up to the fundus, and then gently pressing upwards, so as to push the prolapsed uterus back towards the vagina. The last-formed everted portion of the canal then disappeared under a process of reinversion. This showed how the abnormity might be prevented if the original inflammation were cured in the early stage, and if the uterus were properly supported in its normal position.

Dr. Sims, in reply to Dr. Aveling, said he left the undenuded portion of mucous membrane near the cervix uteri, because this was the most dependent part when the patient was lying down. In reply to other speakers, he said that no one method of operating could be applicable under all circumstances to all cases. If there was a lacerated perineum it was proper to restore it. If there was elongation of the infra-vaginal portion of the cervix, then the proper operation was amputation of the cervix, and this would probably be all that was necessary in such cases. His friend Dr. Eastlake had shown him a case of procidentia two or three days ago, where it depended entirely upon hypertrophic elongation of the lower segment of the cervix, which projected at least two inches below the insertion of the vagina. This patient had been the rounds of many of the hospitals in London, and nothing had been done for her. The perineal operation and his own method were each equally powerless to remedy such a case. Amputation here was the only resource. He had the pleasure of operating on a patient of Dr. Henry Bennet's, a few months ago, where the procidentia depended upon hypertrophic elongation of the cervix. The operation of amputation alone cured the case. In the majority of cases of procidentia—in all those where a vagina-cystocele formed the first stage of descent—he was satisfied that the operation he had described, viz., that of narrowing the vagina by forming a longitudinal fold of its tissue on the anterior surface, reaching from the neck of the bladder to the neck of the womb, would be one of the safest, simplest, and best. As to mechanical means for such cases, he had seen the instruments fail which Dr. Williams had shown us. He had seen cases in which Hodge's lever, Meigs's ring, globes, discs, and air-balls, were all useless. In some cases Zwanck's pessary would answer; but no instrument could be compared with his simple operation for a radical and permanent cure. Besides this, there were many who could not bear any mechanical apparatus. In old women the vagina was often so delicate and tender that none of the ordinary forms of pessaries could be borne. In such cases, if the patient would not submit to the operation, and could not wear an instrument, there was still one thing left to palliate her condition, and that was she could wear a tampon of cotton, the size of an English walnut. It should be moistened with glycerine, and introduced every morning, to be removed at night.—*Obstetrical Transactions, Vol.* vii., 1866, *p.* 238.

87.—ON THE RADICAL CURE OF PROLAPSUS UTERI.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

[Within the last few years many have attempted the permanent cure of prolapsus uteri by plastic operations on the vagina. Many cases have ended successfully. All cases of course are not suitable. Perhaps the most favourable conjunction of circumstances is to be found in an otherwise healthy woman at the close of the child-bearing period, and from that time up to fifty-five or sixty.]

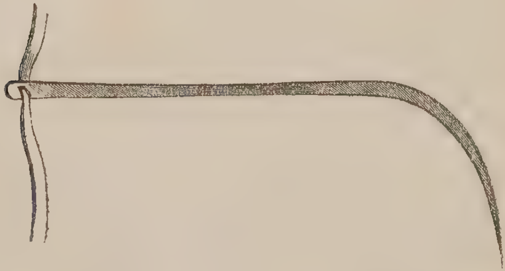
The operation requires the assistance of two or three surgeons (not including the chloroform administrator) and also of a good nurse. It is one which is attended with very little pain for the greater part, but the unavoidable exposure clearly demands the aid of the anæsthetic. The only instruments necessary are a few scalpels; a forceps long and fine at the points, with a tenaculum grip; a duck-bill speculum for stretching the vaginal canal and holding up the uterus; appropriate needles, and the silver wire and quills, which I shall describe.

The patient having been made insensible before the introduction of any of the assistants into the apartment, is drawn to the edge of the bed—a mackintosh being placed under her,—and is placed in the lithotomy position before a good light, an assistant holding each a limb, and a third the duck-bill speculum, while the operator seats himself in front. The parts are closely shaved; the surgeon has then to decide the amount of space which he intends to denude. From the ease with which the lower portions of the vagina can be stripped of their mucous membrane, the temptation to make this denudation pyramidal in its figure is not slight. But upon observing almost the reverse of this figure the future success of the operation really depends—as may be understood from my former article. Let the base of the pyramid, if it be possible, be at or near the cervix uteri, rather than at the vaginal orifice; for there the pressure will be felt the most, and the resistance should be proportionate. I can conceive it possible that if a sound broad surface could be made to unite just below the cervix for a few lines down the canal, it would be far more likely to cure the prolapsus than the most elaborate closing up of the entrance to this canal. I do not, of course, recommend the figure of an inverted pyramid as that which is to be really described; but rather that a square surface should be laid bare, as broad near the cervix as is possible, and extending on either side of the vagina below in such a manner when the sides are apposed as to close up the canal to about half of its former diameter, or even more in cases where there is great relaxation. We thus secure a solid column almost as broad above as at its base, on which the uterus can be supported henceforth.

The denudation is only to be very superficial. With a very keen knife a fine film of tissue may be taken off, and this is quite sufficient to secure adhesion, while it diminishes the risk of the operation and the liability to hemorrhage. Over some parts this is best effected by a species of shaving—as one would cut thin films off a cork; but for this purpose the structures must be made as tense as possible. I must also make one very important practical observation, which is this, that however carefully the parts in this or other plastic operations on these mucous tracts are brought together, union by the first intention will not take place on the *edges* adjoining the mucous membrane. I have very carefully noted this fact. The parts there almost invariably heal by granulation, while the deeper parts become agglutinated and adhere together often without any suppuration whatever. It is therefore necessary to lay bare an amount of surface somewhat in excess of that which we really wish to cause to adhere together. I have nowhere seen any allusion made to this point in the directions for operations on these parts. Plenty of time must be allowed for this part of the operation, and it may be the better because the patient is losing very little blood—altogether not more than a few ounces—and is in no other way injured by a little delay; whereas, if the denudation be done in haste, it is almost certain to be attended with hemorrhage to a considerable amount, and there is the probability of some part being left unbared—the seat of a suppurating sinus hereafter.

The denuded surfaces, being carefully wiped and freed from clots and shreds of membrane, are then laid together. Hitherto they have been recommended to be kept in apposition by two or three deep ligatures of cord, with quill sutures and several superficial interrupted sutures. But I have to introduce a method of effecting this purpose which will, I believe, prove of great simplicity and value. In the stead of cord, I employ a *flat ribbon* of silver, very thin and soft, rather less than the one-sixteenth of an inch in breadth. The needle requires, of course, a transverse slit for carrying it. I have found a shortish needle, well curved, without a handle, in some respects more easy to manage than the ordinary needle in this operation. The thrust necessary to carry the common needle deeply through the tissues (and to get a good hold it is absolutely necessary to make a deep thrust) to the cut edge, and then beyond it and through the opposite side, is very considerable, and cannot be effected without a certain violence. By aid of a good forceps, a shorter needle without a handle can be got easily through and brought out on the opposite side. Fig. 1 represents the form of needle I recommend. It may be useful to note that it is very easy to curve these needles into the various forms required for operations on these parts by gently heating them in the flame of a gas-lamp. They can be afterwards tempered by plunging them when hot into a little sweet oil or water; but the latter requires care and judgment, or the needle becomes very brittle.

FIG. 1.



The flat ribbon of wire being passed through (two or three are generally necessary), is secured in a very simple manner by the peculiar arrangement of the quill which I have adapted to it.

This is shown in Fig. 2. I have made this for my own use in the following manner :—A piece of stout silver wire, of the shape called "half-round," which may be procured at any good tool warehouse, is straightened by a few heavy blows on a flat surface, and then cut into suitable lengths with a file or metal saw. Place two pieces of equal length, say two inches, with their flat surfaces almost in contact, and then with an ordinary soldering tool run a little fine solder for an eighth of an inch down the two ends—no further. In this way we have produced a metal quill with a fine slit running through it, but closed at the ends. The flat ribbon is passed through this slit (formed by the contact of the opposite flat surfaces of the wire), and it may then be made perfectly fast by the simple device of giving it one or two twists ; thus throwing the flat surface of the ribbon transversely across the slit, and making it impossible to pull it through or get it loose. This is done, of course, on both sides, and when the three are secured we have as perfect a suture as can be imagined, For the flat ribbon produces no suppurating tract like the cord or silk, nor does it cut through the tissues like silver wire. If the ligatures be too tight, or the parts swell much, it may be eased by simply untwisting it by a turn or two, and it can be then made just as secure again ; or if it be too loose, it may with the same facility be tightened. And by having a little lateral movement up and down the slit, it has a

FIG. 2.

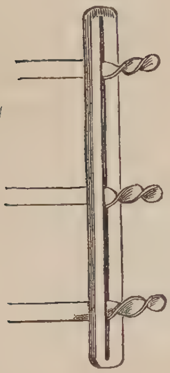
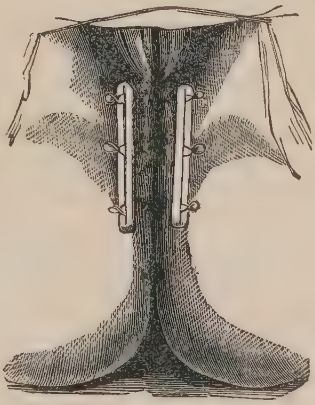


FIG. 3.



self-adjusting tendency which is certainly not without its value. The great convenience and cleanliness of this method of operating is one of its chief recommendations, and it has this advantage in addition, that there is not the same necessity for removing the deep ligatures so quickly as when they are of silk or cord.* A few interrupted sutures of fine silver wire are then necessary to bring together the outer margin of the wound, and these should not be carried too superficially.

I consider it quite unnecessary to divide the sphincter ani for this operation as a general rule, it being important to reduce the action of the surgeon to its smallest limits.

The after-treatment is the same in its general principles as that for rupture of the perineum. But I believe a grain of opium, at first twice a day, and afterwards every night, to be ordinarily sufficient; and I do not by any means approve of the system of drowning the patient with wine, except under very special circumstances. Neither do I find it necessary or expedient to have the bladder emptied oftener than three times in the twenty-four hours. The opium checks the secretion of urine, and there is therefore less occasion for drawing it off. Besides this, the parts are much better left undisturbed as long as possible, especially at first. I prefer using the catheter myself to entrusting it even to the best of nurses. I also greatly prefer washing the wound myself, and this is best done after catheterization. After the tenth or twelfth day the bowels may be opened by a dose of castor oil and an enema.

Before the patient is allowed to rise, astringent injections of alum and tannin should be used twice a day; and a firm bandage with a perineal pad is necessary to be worn for two or three months, until the cure is in fact consolidated. Sometimes a small metallic pessary—the folding wire kind is best—will support the uterus conveniently until the newly-joined structures have become quite firm.—*Lancet, Dec.* 9, 1865, *p.* 645.

88.—ON THE OPERATION FOR THE RADICAL CURE OF PROLAPSUS UTERI, BY CAUSTIC APPLICATIONS.

By Dr. C. H. F. Routh, Physician to the Samaritan Hospital for Diseases of Women and Children, &c.

The operations practised in the intention of mechanically preventing the descent of the womb may be spoken of as of three kinds:—1st. *Kolporaphy*, where they are confined to portions of the vagina, slips of which are removed either by the knife, caustics, or the actual cautery. 2nd. *Episioraphy*, where portions of external skin of the labia or perineum, as well as more or less mucous membrane, are removed. 3rd. M. Huguier's operation where mucous membrane and a portion of the uterus itself is removed.

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* Mr. Hilliard, of Glasgow, has taken some pains in preparing the flat ribbon for me and the needles and quills; but I prefer my own home-made, ruder tools, since they can be adapted to the differing requirements of different cases.

The following cases are examples of episioraphy, only strong caustics were applied, and when the sloughs had come away other parts were brought together by quill sutures.

The caustic I used was strong nitric acid. In this there is nothing new, as it was used by Benjamin Phillips in 1839. The nitrate of mercury having been used by Langier in the same year in France.

The quill sutures usually employed by Dr. Davidson in 1838, Gedding in 1839, I. B. Brown, Dr. Savage, and a host of English operators that have followed in their track during late years, when used with ordinary string, have the advantage of being very easily applied or removed, and the amount of tension to bring the edge together can be easily regulated. But they have one disadvantage and it is, that if kept longer in than the second day, they make a very large lateral hole in the side of the perineum, which is much enlarged by the sloughing which invariably succeeds. So far, convalescence is delayed. To prevent this result, some have contrived to place between the quill and the skin a thin metallic plate, with a narrow slit along its centre, so as to keep the two parts of the thread close together. This is decidedly an improvement, and there is no doubt that, if the ordinary ligature thread is used, it is the better plan. To obviate the inconvenience of thread quill sutures some years back, with the assistance of Mr. Coxeter, I devised two metallic clamps, so constructed that wire may be used instead of thread, and, by one turn of a key, secured *in situ*. The wire used in this manner may be single, and thus the hole made through the perineum is small. The wire is introduced through a circular needle, or it may be used singly without a needle if one point of it is tempered in steel and made very sharp. This last plan has been tried by some with success, although, in such cases, they do not use the clamp, but merely bend the wire after having pressed it through an ordinary wax bougie. The advantage of the single wire is that it may be kept in longer: thus I seldom remove it till the beginning of the fourth day. By the seventh or eighth the superficial may be safely removed. I may remark that, in the usual plastic operation by the knife, when putting on the interrupted wire sutures in the parts anterior to the clamp, I put them on deeply too, only just anterior to the clamp: a plan which, I believe, was first practised by Mr. Brown.

I was led to use caustics because I had felt there were a class of cases in which the knife was dangerous, and, although I now find I was anticipated by Mr. Wells in 1858, my first experiment was at least undertaken in total ignorance of his previous experience.

The operation, although a simple one, is always accompanied with a great loss of blood, and it may occur that owing to the patient's age, the varicose state of the veins about the parts, and the general debility of the patient, the loss of blood, or the use of chloroform, may become dangerous contingencies. In these cases, the first part of the operation may be performed by caustics, and the cure is as complete in the end, although, possibly, not so speedy.

[The patient in the following case was 72 years of age, and in very poor health.]

The patient being very weak, and the aspect of the parts leading to the impression that there would be a good deal of hemorrhage if the ordinary plastic operation was performed, I preferred the employment of caustics. The womb and parts being replaced, the strong nitric acid was painted over a portion of the external skin, extending upwards over the mucous membrane of the vagina, about 1½ inches upwards, and for the lower, half of the external opening. No chloroform was given. The pain at first was considerable, but entirely subsided after four hours. Poultices were applied every three hours, and a suppository of opium ordered if the pain continued.

From this time up to May 3, she appeared to be doing well. On inspection, however, the separation of the sloughs was found to be proceeding very slowly. The bowels were constipated, and there was some abdominal pain. She was ordered a purge. On inquiry it was found that the nurse had neglected the application of the poultices. These were now ordered to be regularly applied both within and without the vagina. On the 5th the slough had not come away, and two or three spots were again touched by nitric acid. On the 9th the sloughs had completely separated. The granulations being well washed by a stream of warm water, the parts were brought together by clamps, and two deep sutures applied. A catheter was left in the bladder. About half an ounce of blood only was lost by the introduction of the deep sutures. No superficial sutures were applied. In the course of the 11th some sickness supervened, which was relieved by creosote and salines. There was considerable vesical irritation, which ceased on the removal of the catheter. The deep sutures and clamps were removed on the 12th. The union was complete and the perineal cushion very good.

Some days subsequently (18th), the bowels having been kept constipated, a sort of diarrhœa supervened, and in the passage of a large fæcal mass she felt as if something had given way, and a small quantity of blood passed out. This was at the time attributed to piles. On the 19th, however, it was found that a great portion of the new perineum had given way. The operation was therefore performed a second time. The result of this second operation was much more satisfactory, union again taking place posteriorly, and only deficient a little anteriorly. This point had again to be touched by nitric acid and brought on the separation of the slough by stitches. This case ultimately did very well, the uterus receding backwards, and the vulva opening becoming smaller, contracting gradually independently of the new perineum.

This contracting effect of caustics at this part has not, I believe, been insisted upon before, and I make no doubt that had superficial sutures been applied as well as the quill, the case would have done well after the first operation.

I am informed by Mr. Spencer Wells of two cases in which he had used caustics for the purpose of forming a granulating surface, which, when brought into apposition, might unite as readily as the raw surfaces usually made by the knife. These cases occurred in private practice in the spring of 1858, the caustic being used because the ladies were very unwilling to submit to any operation requiring the presence of assistants, and were fearful of the knife. In one case nitric acid was used, and the granulating surfaces, after the separation of the slough, were brought together by three silver sutures. Union took place, but it was very superficial, and only served to keep up the prolapse by the assistance of a T-bandage and perineal pad. In the other case, the acid nitrate of mercury was used, a horseshoe piece, an inch and a half in depth destroyed, and poultices used until the slough separated on the sixth day. On the seventh day the granulating surfaces were brought together by the quilled suture, and a very firm thick cushion was obtained. Mr. Wells has informed me that he had heard very lately from this patient, and that the result has been most satisfactory, a prolapse of twelve years' standing having been completely retained within the vulva. He did not use chloroform in either case, but applied Dr. Arnott's frigorific mixture before the acid. In both cases there was a good deal of pain for two or three hours after the application.

The employment of caustics in these cases proves it has some advantages :—

1. It needs a smaller staff of assistants.
2. It precludes a loss of blood.
3. It causes contraction of the parts around the vulva, and relieves the uterine congestion.
1. In hospitals, where we have proper tables, dressers, nurses,

&c., in abundance, the disadvantage of few or many attendants is not so much felt, but in private practice it is not so. Ladies do not like to exhibit to several men. Here one attendant suffices, and even, if desired, none but the operator need be present.

2. It precludes the loss of blood; and this is really a matter of great importance. The state of weakness to which many patients after the operation are reduced is very great. The blanched look of the women for several days after it is painful to contemplate. Few practitioners have operated by the knife upon female organs of generation without being often surprised at the great amount of hemorrhage which will often follow the slightest cut of the knife. Nor is this all. Secondary hemorrhage after this operation is not rare, and this in cases where the known skill of the operator left no doubt as to the accurate manner in which it had been performed. Many of those weak females with flabby hearts before alluded to, as cases in which the administration of chloroform is dangerous, are precisely those in which loss of blood is so injurious. Hence, one of the reasons why recovery in these instances is often so slow, even after union is completed. The expense, moreover, of keeping up such patients by wine is felt to be a heavy tax on some hospitals. But there are, besides, cases in which operation by the knife would be exceedingly hazardous. One such case, I remember, some time back, and in which, upon consultation, all idea of operation was in consequence abandoned. It was the excessively varicose condition of all the veins about the vulva and upper part of thighs. True, this state of things might have been in some measure relieved by drastic purgatives, but even then the hemorrhage which might have followed during the operation seemed entirely to preclude the attempt. Aged patients, also, cannot afford to lose blood, particularly if weak and long ill-fed, as my patient had been.

3. The application of the caustic, even though it be not actual cautery, has the tendency to cause all the parts around to contract. It is my belief that the mere application of a strong caustic so as to destroy deeply, would in time so contract the opening that it would not be necessary to bring the parts together by sutures at all. However, in great looseness of the parts, it appears more prudent to use them, and the opinion just expressed requires direct experiment to prove it.

There were some disadvantages met with in the three cases which require to be noted. 1st. The pain endured by the patients for three or four hours after the application. This is, of course, an objection, but measures could be taken to overcome it by opiates. 2nd. In two out of my three cases a small sinus existed at the lower portion. I am at a loss to understand why it should occur here, but as it was not observed in Mr. Wells's two cases and my first, it was probably accidental. At any rate this is a mishap readily rectified, as it was in my second case. But if the edges are paired and brought together by wire ligatures, the hemorrhage is inconsiderable, and the cure certain. I prefer not bringing the edges laterally, but from before backwards, the threads being passed from the rectum externally. In one very bad case in which I operated and in which the entire external sphincter and part of the internal were lacerated the plan succeeded perfectly. We should remember that the disposition of parts is to this kind of union, and it is remarkable how, after a time the sphincters seem to acquire strength and greater power. One or two more practical hints and I have said all I wish to say on this operation.

1. As to the keeping the catheter within the bladder, I think it unnecessary, if the top of the wound is kept guarded by a piece of lint the urine will do little harm, the cut edges being brought together by apposition and not exposed to the contact of urine; however, it avoids all chance of cystitis, so common and so distressing a symptom after the operation, when the catheter is kept in.

2. The vagina should from the first be frequently washed out with cold water, I like it done three or four times in the day, and at night if need be, and especially after the patient makes water. The operation is very grateful to the patients, and keeps the parts always sweet and free from much pain. The ordinary chlorate of potash and lavender lotion is the best I know.

3. I think it wise to remove the deep sutures by the third or fourth day at the latest, the superficial should, I think be removed about the seventh or eighth, but as a precautionary measure the superficial should be kept in until the bowels have been opened by some laxative like pulv. jalap, co., or bitartrate of potash, which produce watery movements, and preceded by an injection of water. I think, also, it is a great improvement of Mr. Brown's to apply the superficial sutures deeper.

Lastly, admitting the disadvantage of sloughs formed by the caustic applied, and the irritation set up by the discharges from the large ulcer formed for the time being, and often so difficult in this situation to heal when once made, this very ulceration has often a very great and favourable influence on uterine congestion. I believe this result obtains when the caustic is used in the perineal operation in lieu of the knife more effectually on the more neighbouring parts. Then once the perineum has been formed and the two sides brought together into apposition by quilled sutures the uterus must needs become absorbed, because the two causes before referred to, the invaginating action, and the greater weight pulling down at the vaginal end, and from filled bladder, &c., no longer exist. Other reasons have been mentioned by other authors as co-operative, but as these will be found referred to in text books and Dr. Savage's new work on the "Female Perineal Organs," I do not refer to them here.

In conclusion, I may say I do not propose the use of caustics as if I wished them to supersede the ordinary plastic operation, which I have practised and will still continue to do so in most cases, but only in the special instances referred to in this paper when the use of the knife is contra-indicated.—*Medical Mirror, March* 1866, *p.* 121.

89.—CASES OF POLYPUS UTERI: WITH REMARKS.

By Dr. J. G. Swayne, Physician-Accoucheur to the Bristol General Hospital, and Lecturer on Midwifery at the Bristol Medical School.

[The two following cases of polypus uteri are similar in many respects, and are worthy of notice; because in both the polypi were larger than usual, and it was, in consequence, found necessary to modify the operation proposed for their removal.]

Case 1.—On May 18th, 1865, Mr. Parsons requested me to see an unmarried lady, Miss P., of about 38 years of age, whom he had been attending for six weeks. He stated that she had had a tendency to menorrhagia for the last two years, but that he was first consulted on account of a rather profuse hemorrhage, followed by a copious and very fœtid discharge, from the vagina, and accompanied with some pain and aching in the loins. On examining, he detected a large tumour in the vagina, which he was inclined to consider a cauliflower excrescence; but, as he did not feel quite satisfied on that point, he requested a consultation with me about it.

When I first saw her, her general health did not appear to have suffered much, although she was rather pale and exsanguine in appearance. The discharge from the vagina was so offensive, that it rendered the room in which she was very disagreeable. On examining *per vaginam*, I felt, just within the os externum, a tumour, apparently of about the size of a large orange, filling the vagina and distending it considerably. The lower portion of the tumour was rather soft and irregular, as if shreds of it were partially detached and in a state of decomposition; but it had not the peculiar soft spongy feel of a cauliflower excrescence, nor did it bleed so readily on being examined. When viewed through the speculum, its surface appeared of an ashy gray colour, and not bright red like a cauliflower excrescence. On passing my finger up as far as I could reach around it, I was able to feel the pedicle of the tumour, which seemed to be about as thick as my middle finger and more firm and fibrous than the rest of the tumour.

After a short consultation, we agreed that it was a large polypus growing from the os uteri, and we decided on an operation for its removal.

This I accordingly performed on May 23rd. The patient being placed on a firm table and lying on her back in the lithotomy position, I endeavoured for some time to draw down the tumour beyond the os externum in order to excise it; but the vaginal orifice was rather narrow, and although I made repeated efforts both with a vulsellum and an ordinary obstetric short forceps, I could not bring the polypus out beyond the os externum without using more force than I thought justifiable. The tumour also gave way in two or three places, and in consequence of this a very copious hemorrhage ensued. At least a pint of blood was caught in a basin, besides a good deal which saturated her clothes and ran on the table. I therefore, as she was becoming faint, though it prudent to desist from further efforts; and contented myself instead with passing a firm ligature of whipcord round the neck of the polypus by means of Gooch's double cannula. As soon as the ligature was tightened, the hemorrhage entirely ceased.

When we visited her on the next day, the polypus did not feel so firm as before; and, by getting three or four fingers of the left hand behind it, I succeeded in dragging it completely out. The double cannula and whipcord were then removed; and, instead of it, I passed a needle armed with a double thread of strong silk through the pedicle. This was tied on each side, and the pedicle cut through close to the ligature. No unfavourable symptom followed the operation.

On examining with the speculum two days afterwards, the ligature was observed to have come away, together with the remains of the pedicle, and to be lying loose in the vagina.

The patient made a good recovery.

Case 2.—On August 7th, 1865, Dr. Crocker requested me to see with him an unmarried lady (Miss H.), aged 40, who had been under his care for some weeks. She had been suffering from repeated hemorrhages from the vagina, which had brought her into a very weak state; and also from retention of urine, necessitating the daily use of the catheter.

On making a vaginal examination, I felt, just within the os externum and pressing very much on the meatus urinarius, a large ovoid tumour, which filled the vagina and presented most of the characteristics of an uterine polypus. It was firm and fibrous to the touch, and bled freely when roughly handled. I could not reach any pedicle with my finger; but by using the uterine-sound and passing it round it, I could make out that it was attached to the uterus by a narrow stalk. The discharges from the vagina were not offensive, and no kind of decomposition seemed to be going on in the tumour. As the tumour was evidently a polypus, we at once decided on its removal.

This I proceeded to do on August 21st. Having a vivid recollection of the hemorrhage which followed my attempts to draw down the polypus in the former case, I began by placing a ligature round the pedicle by Gooch's double cannula. When the ligature was tightened, it appeared, in a great measure, to control the hemorrhage from the tumour. I then applied a short obstetric forceps, and having obtained with it a firm hold of the tumour, I made several endeavours to draw it beyond the os externum; but I was completely foiled in my attempts by the contracted state of the entrance of the vagina, which I felt that I could not overcome without undue violence. I therefore tightened the ligature, and left her.

I saw her next day, and again tightened the ligature. The tumour had now become less firm and resisting; and, from the offensive smell of the discharges, appeared to be undergoing rapid decemposition; but yet I found it impracticable to remove it.

On the day following, the whipcord broke whilst I was tightening the ligature. I then made several attempts to draw the tumour out by the obstetric forceps; but still failed, owing to the great resistance of the os externum. No hemorrhage, however, resulted; but so much soreness of the parts was occasioned thereby, that she begged me not to make any further attempts for some days. I therefore merely directed her to inject twice a day into the vagina about a pint of water containing Condy's solution.

On the 29th (eight days after the ligature was first applied), whilst using this injection, a powerful bearing-down effort was induced by the stimulus of the fluid, and the tumour was suddenly expelled into the chamber-utensil. It was large and pear-shaped, and appeared to have been attached by a rather narrow pedicle.

I saw her again on September 1st. She was then apparently going on well. I examined her with the speculum, and observed a small remnant of the pedicle just within the os uteri. This I touched freely with nitrate of silver.

About a week after the pólypus came away, she had an attack of phlegmasia dolens in one leg, apparently in consequence of absorption of the putrid discharges. The attack was not severe, and soon passed away under the usual treatment. With this exception, she made a good recovery.

Remarks.—There are two or three points in these cases which are worthy of notice, on account of the bearing which they have upon the pathology and treatment of polypus uteri. The source of the repeated hemorrhages which characterise this disease, has long been a *vexata quæstio;* some being of opinion that the bleeding proceeded from the uterine mucous membrane, whilst others have thought that it came from the polypus itself. The history of the two cases above related would seem to favour the latter hypothesis; because, in both, copious hemorrhage ensued when the surface of the polypus was torn or even roughly handled, and this was at once arrested when a ligature was applied round the pedicle; nor could it be re-excited by any handling or dragging of the tumour immediately afterwards. Such an effect is too instantaneous to be accounted for (as has been stated by Dr. West), on the supposition that "the ligature interrupts the vital relations between the tumour and the womb, and thus renders the polypus a far less powerful excitant of the uterine mucous membrane than it was before." On the other hand, the amount of blood which flows at various times from the uterus seems to bear no relation to the size or vascularity of the polypus, as it ought to do if it proceeded entirely from it and not from the uterine mucous membrane. In a case, for instance, where I removed by excision a polypus not larger than a nut, the hemorrhage previously had been quite as great, if not greater, than in either of the two cases I have related above, and yet the excision was unattended with any bleeding. On the whole, I am inclined to think that the hemorrhage may proceed from both these sources, the quantity furnished by each depending very much upon their respective vascularity and a variety of other circumstances

With respect to the operation required for the removal of polypi, the ligature was at one time very generally adopted for this purpose; but it has of late years fallen, I think, very deservedly into disrepute, excision being now more frequently adopted. The old idea, that the removal by ligature is a much safer operation, has been effectually disproved by facts. The removal by ligature is a tedious operation, for frequently several days elapse before the tumour sloughs off; it is a dirty operation, for, all the time the tumour is decaying, a horribly offensive discharge is issuing from the vagina; and it is a dangerous operation, because the putrid *débris* of the decaying polypus may be absorbed and set up uterine phlebitis and peritonitis. I shall not readily forget a case of this kind which occurred some years ago at the Bristol General Hospital. A woman was admitted who had a polypus, of about the size of a hen's egg, growing from the uterus. A ligature was placed around the pedicle by Gooch's double cannula; and the operations was so effectual, that the polypus came away in three days. But, before a week had elapsed, the woman died of metro-peritonitis. The danger of this accident is, I believe, much greater when the polypus is attached, as it was in this instance, to the mucous membrane lining the cavity of the uterus, than when it grows from the lips of the os. Many such untoward events have occurred; but, as Dr. West well remarks, "there is nothing that places the dangers of this operation in so strong a light as the fact that, out of twenty cases of removal of fibrous polypi by ligature, recorded by a most strenuous defender of that operation, Dr. R. Lee, nine, or almost half, had a fatal result—a mortality more than double that of the operation of lithotomy, as high as that which occurs in placenta prævia, and higher than the mortality from malignant cholera."

In the two cases described this evening, I was compelled by circumstances to employ the ligature; but in one of them I was able to excise the tumour on the next day, whilst in the other I found it impossible to do so; and the risk which the patient incurred in this instance is shown by the attack of phlegmasia dolens which supervened.

The operation of excision, which is in every other respect so preferable to the ligature, has been objected to on account of the risk of hemorrhage which attends it—without, as Dr. West observes, any sufficient reason. He states that, according to the experience of Velpeau, Lisfranc, and Dupuytren, such an accident is very rare; and that, in his own experience, twelve cases of excision were unattended with any hemorrhage whatever. My own experience, does not prove that the operation is quite so free from danger in this respect, although it is very limited, as I have excised polypi in but seven cases, yet in one of these considerable hemorrhage ensued. I removed *in situ*, by a pair of curved scissors, a small soft polypus, not thicker than the first joint of the forefinger, and was astonished by observing a copious gush of blood from the vagina immediately afterwards. To arrest this, I introduced a large speculum, in order that I might cauterise the pedicle; but found it quite impossible to do so, as the speculum, in a very short time became quite filled with blood. I therefore introduced a large sponge coated with gallic acid, and firmly plugged the vagina. This completely checked the hemorrhage, but not before rather alarming syncope had ensued. On examining the polypus, I found that it was supplied with blood by an artery quite equal in size to one of the digitals.

In the first case which I read this evening, an examination of the polypus showed that it was fed by an artery quite as large as the radial at the wrist; and in this instance, no doubt, simple excision would have been followed by alarming hemorrhage.

On account of the risk of hemorrhage, it is better, in all cases where it can be applied, to remove a polypus by the écraseur, rather than by the knife or scissors; and this, could it have been adopted, would have been by far the best operation in the two cases above mentioned. But, in consequence of the large size of the polypi and the contracted state of the vaginal orifice in both cases, I was unable to get at the pedicle so as to use the écraseur or even the curved scissors; and I therefore very reluctantly used the ligature and excision in one case, and the ligature alone in the other.

[Mr. George May, Jun., of Reading, writes in the next number of the British Medical Journal as follows:]

I am induced by the report of Dr. Swayne's Cases of Polypus Uteri, to remind your readers that the contracted vagina in the unmarried female may be rapidly and safely expanded by Barnes's dilators sufficiently to admit the hand. The patient should be under the influence of chloroform.—*British Medical Journal, Jan.* 20 *and* 27, 1866, *pp.* 69, 112.

90.—ON A NEW UTERINE SUPPORT.

By Dr. William Main, Lasswade.

[Perhaps the most generally useful appliance yet known for the relief of displacements of the uterus is Dr Simpson's loop pessary of gutta-percha. Yet there are many cases in which that instrument affords little or no relief though fitted with all the care possible. The instrument here described is perhaps an improvement on Dr. Simpson's.]

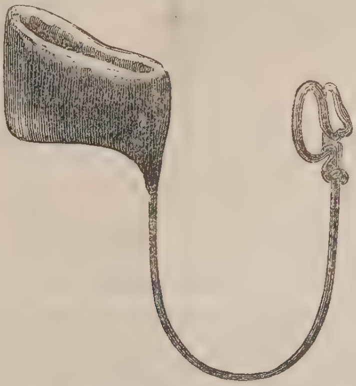


Fig. I.—Side View of Uterine Support for Retreversion

It consists of a circle of gutta-percha, enclosing a copper wire of No. 12 thickness, fitted to receive the os and vaginal portion of the cervix uteri. Connected with this circle is a stem of the same copper wire, a continuation of that enclosed in the gutta-percha, which terminates in a sliding loop. The instrument is very simple, is easily made, and does not cost more than two or three pence. Its application is also very easy. A bandage round

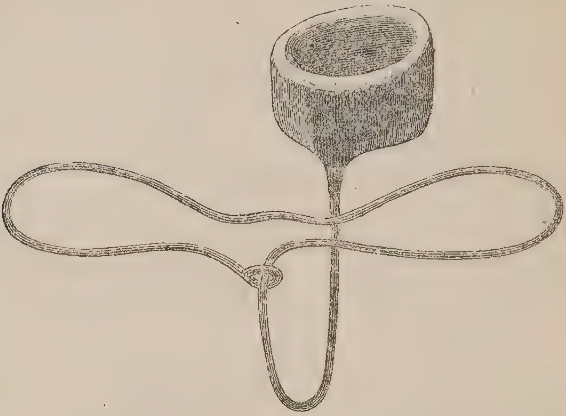


Fig. II.—Front View of Uterine Support for Retroversion.

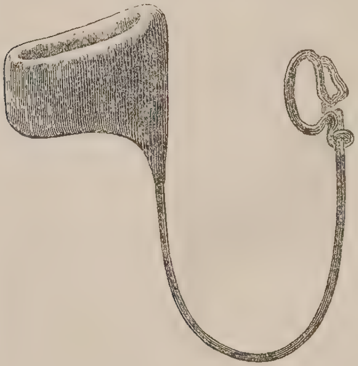


Fig. III.—Side View of Uterine Support for Cases of Anterversion.

the loins, with thigh straps, having first been adjusted, a sound is introduced into the uterus, and its position rectified. The gutta percha circle is then passed over the sound, which acts as a guide, and is pressed gently upwards till it receives the os and cervix. The stem is then bent to fit the vagina, and, being sustained there by the fingers of the left hand, the remaining portion is carried forwards to the upper edge of the mons veneris. The running limb of the terminal loop is now brought to this point, and secured to the main stem by a piece of tape. It now only remains to bend the loop into two semi-circular horns, one to run along the lower part of the abdomen on either side, immediately above the groins, and fasten these horns to the abdominal bandage by tapes or stout pins. The instrument is now secure, and, when properly applied, it has never in my hands given rise to pain or irritation ; but, on the contrary, has invariably afforded relief to all the annoying uneasiness of a displaced uterus, and permits of any movements, such as walking, sitting, or stooping, with perfect freedom.

I have had an opportunity of trying the instrument in a few cases of prolapsus, retroversion, and anteversion of the uterus, and have found it equally serviceable in all of these, a slight change in the form of the gutta-percha circle being all that is necessary to suit the different kinds of displacement. In cases of simple prolapsus, the upper margin should be horizontal. Again, in those of anteversion, the anterior part of the circle should have a raised cushion of gutta-percha, so as more effectually to support the uterus in its proper place ; while in cases of retroversion, the cushion or bolster should be on the posterior part of the circle, with a similar object in view.

The instrument may be made large or small, deep or shallow, to suit the circumstances of each particular case ; and should the patient be large or fat, a wire of greater thickness than No. 12 may be substituted with advantage.

I may also mention that I have found the copper wire frame, without the gutta-percha circle, very useful when using Dr. Simpson's galvanic pessaries. By taking merely the loop and stem, and on the end of the latter fixing the galvanic pessary with a little heated gutta-percha, it can be retained perfectly in its position in the uterus, while, at the same time, that organ can be kept at any desired angle by bending the stem ; the loop end of the wire being fixed to the bandage, as before suggested.

As an illustration of the usefulness of these instruments, I append a short account of two cases. First, a case of anteversion.

E. T., aged 24, a servant girl, had been incapacitated from work for some years in consequence of dysmenorrhœa, with stricture and anteversion of the uterus. Intra-uterine galvanic pessaries were introduced; but by no possible means could they be retained, the uterus expelling them as soon as the support of the finger was withdrawn. The gutta-percha platform support, india-rubber-balls, and a sponge, were all tried in vain. They failed to keep the pessary within the uterus. Dr. Simpson's wire pessary was now tried, and it gave complete relief; but after a few weeks it caused so much irritation that I was obliged to withdraw it, when I found the uterus had become very much enlarged. In a short time the uterus was discovered to have resumed its former anteverted position, and I then began to use the loop pessary; but it gave little or no relief. Some modifications of the instrument I now show were afterwards tried, with encouraging results; and she has now been wearing one of the present form for some months, with complete relief. A day or two ago, I passed a sound, nearly straight, up to the fundus of the uterus.

How far she may utimately recover it is impossible to say. At present, menstruation is not quite regular, and she suffers from frequent disordered function of the liver, but can walk about with perfect ease and comfort.

The next case which I shall relate is one of retroversion.

M. C., a young woman, 25 years of age, had suffered severely from dyspepsia, for seven years before coming under my care,—vomiting a large quantity of mucus, and a small portion of her food almost daily, for nearly all that period, according to her own and her mother's statement. Her menstruation was irregular, painful, and not free; and she suffered from pain and weakness in the back, along with other uneasy symptoms which usually accompany this condition. After careful inquiry, I came to the conclusion that the uterus was the source of her distress, and warned her friends that local treatment would most probably be required, but that, in the meantime, medicine could be tried. Bismuth, nitrate of silver, nux vomica, lime-water, &c., were tried, with temporary good effects, but at every menstrual period all her symptoms were greatly aggravated. On examination, per vaginam, the uterus was found retroverted, and the passage somewhat constricted. The introduction of a galvanic pessary greatly relieved her dyspeptic symptoms within twenty-four hours, though a little local irritation was experienced. Under the use of the pessary for a short time, and without medicine of any kind, menstruation became quite regular and natural, and the functions of the stomach were completely regained. I now thought we could possibly dispense with the pessaries altogether, and withdrew the one she wore; but within twenty-four hours vomiting returned as bad as before, accompanied by all her other distressing symptoms. The gutta-percha loop pessary was now tried, but with only very partial relief;

then some modifications of the present instruments, which were found to give relief so long as they retained their proper position. On becoming displaced or withdrawn, however, the vomiting almost immediately returned. She has now been wearing a pessary similar to the one I have shown you for about four months, with perfect comfort, and with relief to all her symptoms. She can walk any ordinary distance, work in the garden, stoop, &c., with ease, and has grown comparatively stout and ruddy on good food, air, iron, and cod-liver oil; and my only anxiety now is to find whether she will ultimately be able to do without the pessary. At present, I fear, she cannot; for about a month ago I withdrew it, in the hope that she would remain well; but I was disappointed to find that the vomiting returned in two or three days, and continued to recur as usual till the instrument was replaced.—*Edinburgh Med. Journal, Dec.* 1865, *p.* 545.

91.—ON EXTREME FLEXIONS OF THE UTERUS.

By Dr. Henry G. Wright, Physician to the Samaritan Hospital for Women.

The distinction of those uterine deviations formerly comprised under the general title of ante- or retro-version into true version (where the whole organ is bent like a pear on its stalk) and of flexion (where the bend is in or above the uterine cervix) has come to be very generally accepted, and is not without its practical utility.

It is to the latter class of cases, where there exists some considerable alteration of the natural angle which the cervix uteri forms with the body of the organ, that the following remarks are restricted; and more especially to those extreme conditions of flexion where the uterus is, to a greater or less extent, folded on itself. Where this displacement occurs backwards (its most frequent direction), the facilities for physical examination presented by the prolongation of the vaginal cul-de-sac behind the cervix allow of its more ready recognition and replacement. Notwithstanding the advantage thus afforded, the diagnosis should be very carefully formed; for there occasionally occur in this situation tumours which present to the touch characters very like those conveyed by a retorted fundus uteri. But with ordinary care, there is rarely any difficulty in arriving at a correct diagnosis.

Retroflexion of the uterus in the utmost degree—with the fundus forced down in the fossa of Douglas and the cavity of the organ bent like a hook—may be considered as a type of extreme intrinsic displacement. In such cases the cervix may but slightly deviate from its normal situation, or even occupy its natural position relative to the vagina; yet the protruding posterior lip of the os uteri, and the elongated linear orifice, curved with its convexity forwards, should at once induce suspicion that there has ensued some disarrangement of the natural order of uterine place.

The suggestion that a uterus so extremely displaced (and affording distinct etiological evidence of results constantly met with only where it is thus distorted) should be treated as for some supposed inflammation, without regard to the deviation of place, is, I firmly believe, just as unreasonable as it would be to treat a strangulated hernia by the application of leeches, without attempting any reposition of the bowel.

The use of some intra-uterine support indubitably owed its origin to the proved inefficiency of ordinary methods of treatment in certain extreme cases of uterine versions and flexions. In the latter cases the fundus could be raised by careful manipulation, the cervix brought well backwards, and the uterus—long unnaturally distorted—be thus replaced, with immediate relief to the patient. But too often this good result was only of temporary duration. The fundus gradually rose and reclined again, and the urging of intestinal action caused it to resume its vicious position. There would thus be successively brought about three distinct conditions of mechanical displacement. 1. The whole organ is first turned on its axis, producing a partial retroversion. Then there gradually ensue the stages of deviation, which are ranged under the head of flexions—viz.: 2. A cornuation of the uterus, the organ being unnaturally *curved* (most frequent in cases of anteflexion). 3. The true flexion where the body is bent on the cervix at a sharp angle. The first stage—that of retroversion—is not commonly met with where the neck is empty, since the conditions under which it most frequently happens are just those which lead the patient to delay seeking advice. The following case illustrates such a state:—

Mrs. ——, aged 27, confined a year, suckling the child for only six months. After the first month she began to move about; but felt so much weight and forcing in the body that she was glad to take to her couch again. Carriage exercise increased her suffering, until she had to give up every attempt at exertion, since it induced such increase of pain. The action of the bowels always produced suffering. Latterly some blood usually passed with the motions, and she came to consult me in the belief that her sufferings were due to internal piles. The uterus was large and soft, but not tender—just in that condition so often found after a difficult labour, where the subsequent involution has been inefficiently effected. The cervix was low down in the vagina, and the organ directed somewhat backwards, with a slight reclination of its fundus. Reposition by means of the uterine sound (which entered to a depth of nearly three inches and a half) gave immediate relief. There was no recurrence of the displacement or of the bleeding; the patient became again pregnant, and has felt no further inconvenience.

In even more advanced reclination, if careful replacement be effected just prior to a menstrual period, and great caution exercised throughout that time, permanent relief may sometimes result. For the reparation of uterine tissue, which is determined during each catamenial period, supplies a valuable auxiliary in the treatment of uterine disorders of all kinds, and always requires to be held in mind.

Moreover, in these extreme cases of retroflexion even where the displacement recurs again and again, it often happens that a cure may be effected by extra uterine supports—setting aside the necessity for resorting to any intra-uterine appliance especially in cases where some neoplastic thickening of the posterior uterine wall induces retroflexion by its weight, and menorrhagia by its presence. Here reposition, followed by the adaptation of a fully inflated air-ball pessary, as advised by Valleix, in order to sustain the uterus high up in the pelvis, mechanically prevents recurrence of the displacement, whilst appropriate treatment is pursued for restoring the general health. Notably this was the result in the following cases, which serve also to illustrate the very diverse physical conditions which may produce the same disorder of uterine place.

1. A widow lady, aged 46, of healthy family, strong constitution, and full habit, suffered from such extreme menorrhagia and urging dragging back-pain that she had been almost entirely confined to her couch for three years. The original cause was clearly traced to sudden check of menstruation by exertion and night exposure during a fire. There followed gradually increasing dysmenorrhœa and menorrhagia, her sufferings greatly increasing at the menstrual periods, which were both too frequent and too free. The recto-uterine space was occupied by a rounded tumour, which, by using the sound, was distinguished to be the thickened posterior wall of the retroflexed uterus. This was carefully replaced, and a large air-ball adapted as high up as possible and fully inflated. Great local relief ensued; the health gradually improved under general treatment, though the progress was but slow. The patient has now been upwards of a year without any instrument or recurrence of the suffering, and walks and works and makes long journeys without discomfort.

2. E. D., aged 28, married, very thin and debilitated. A week after birth of her last child (in India, five years ago) she left her bed to attend her husband through a severe illness.

Has ever since suffered from dysmenorrhœa and symptoms of uterine displacement ; being unequal to any exertion, and presenting the usual complications met with in Indian cases—leucorrhœa, hepatic congestion, constipation, severe sympathetic headaches, enfeebled digestion, oxaluria, &c. The fundus uteri was felt low down behind the cervix, and carefully replaced. The long enfeebled and attenuated organ measured three inches and a quarter, and tended to fall forwards, just as it had previously bent backwards. Here the ball-pessary sufficed to maintain position, whilst the general health was improved by tonic treatment. Two months after, she could walk three miles without inconvenience, the pessary having been left off after the first menstrual period had passed.

But there occasionally come under notice cases where no extra-uterine appliance does any good, and where the ingenious curved supports devised by Dr. Hodge of Philadelphia, and modified by Drs. Priestley and Marion Sims, cannot be borne because of the irritability of the peritoneal pouch that descends behind the uterus ; and where the numerous other ingenious devices recommended prove equally inefficacious, the replaced fundus returning to its retroflexed position, and all the old suffering coming back to the patient, on any effort or exertion. It was the occurrence of such desperate cases which undoubtedly led Amussat, Velpeau, and Simpson, to resort to the use of some intra-uterine means of support, as affording a fair probability of relief. Of the instruments devised, that of Sir James Simpson is the only one worthy of note. Its success in his hands at once attracted attention, and the intra-uterine stalk-pessary was extensively tried. That in a considerable number of cases there ensued very serious results—metritis, pelvic cellulitis, peritonitis, and death, all attributed to its use—cannot be doubted. And individual writers in this country have joined chorus with the speakers at the Imperial Academy of Medicine who so vehemently denounced the use of the instrument during the discussion by that learned body of the use of intra-uterine pessaries. But it must be remembered that Dr. Simpson recorded the results of his own practice without making himself responsible for the results which might arise from defective application of the instrument ; and even where its use is most vehemently condemned, there is no attempt to explain the cause of its failure—to ascertain whether it be the stem-pessary that is in fault, or the principle of intra-uterine support that is erroneous, or the operator that is to blame.

The evil results which have indubitably ensued in many cases where instrumental intra-uterine support has been attempted are, I believe, chiefly attributable to two causes :—

1st. The method of using the uterine sound ordinarily pursued to produce reposition is very liable to seriously hurt the the uterus.

2nd. A firmly supported intra-uterine stalked pessary must produce mischief unless used with very exceptional skill, and is calculated to perpetuate or increase any injury caused during the preliminary replacement of the retroflected uterus.

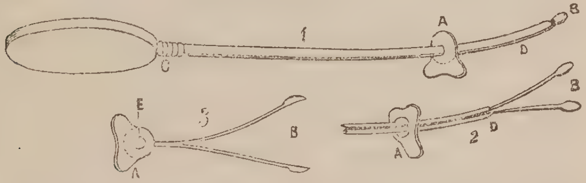
This latter proposition explains itself; when it is remembered that the uterus, whether healthy or diseased, is never absolutely at rest, moving downwards with every inspiration, and with every effort which induces descent of the diaphragm—as coughing, sneezing, laughing, &c. It is influenced as to its position by the condition of the adjacent organs, and by any strain thrown on them. It especially resents continued pressure against any point of its internal surface, and such local irritation is pretty certain to determine expulsive effort. If this effort fail, and strong, unyielding pressure on the delicate intra-uterine membrane be maintained, there ensue results ranging through that series of severe symptoms noted as following the employment of the intra-uterine pessary; for its due adjustment implies that an ivory or metal stalk lies within and holds up the womb; this being firmly maintained in position by a vaginal stem connected with a well-adapted external support. Thus the fundus of the replaced uterus is at every downward movement checked by the presence of an unyielding, inelastic intra-uterine stilette, the point of which always impinges on the same spot. The success of the instrument in the master-hand of Sir J. Y. Simpson has probably been to his so adjusting the length of the intra-uterine stem that it just sufficed to retain the uterus in position, without impinging against the fundus. But at best it must be a hazardous procedure, and the more so when it is remembered that there already exists an unhealthy structural condition in the majority of cases of chronic retroflexion.

Whichever be the kind of uterine displacement, however extreme its extent, and whatever the proposed plan of treatment, the first indication is to produce restoration of the natural position, if this be possible; for there occur cases where it would be eminently injudicious to attempt any interference, on account of adhesions having been formed, or from other causes. This preliminary replacement usually requires the employment of the uterine sound, and if not adroitly done may itself be productive of mischief, as already noted. The general directions given for the use of the instrument in cases of extreme retroflexion seem very inadequate. The strongly curved sound is to be introduced until it passes to the depth of the uterine cavity, and then turned or twisted on its axis until the fundus is moved forwards. Anyone holding the instrument in his hand and thus rotating it, will at once perceive what a powerful leverage is exercised, and how the uterus must be dragged into a new condition of lateral deviation whilst undergoing the half-turn that is to bring its concavity forwards, with great probability of injuring the delicate tissues pressed on.

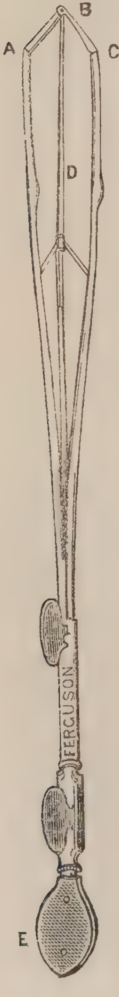
The rule for reduction of all disorders of uterine place is that the organ shall be gently caused to retraverse the course originally passed through on its way from the natural to the unnatural position. In a case of extreme retroflexion, reposition may be readily accomplished according to the above rule. The patient is placed on her side, doubled together so that the body lies across the couch and the knees approach the chin. The curved sound introduced with the concavity backwards is gradually passed round the flexure to the full depth of the uterine cavity, whilst the forefinger of the free hand gently raises the fundus, helping, as it were, to fit the displaced part on to the end of the instrument within the cavity, so that it may obtain full command of the organ. The next stage is that of replacement, sweeping the fundus uteri steadily upwards, forwards, and somewhat downwards. The intra-vaginal finger is shifted so as to press on the anterior part of the cervix, and prevent the instrument receding. Then the handle of the sound is carried gradually backwards until it lies within the fold of the nates, that portion of the instrument engaged in the cervix constituting the axis. Thus the point of the sound lifts the fundus uteri upwards and forwards. If now, retaining the same axis of movement, the handle be raised, and swept round in a semircircle until it comes down behind the thigh, there is thus acquired the power of continuing the tilting forwards of the fundus uteri until its natural position is attained, without causing any other deviation of place than originally occurred in the progress of displacement. When replacement has been thus accomplished without undue drag or the use of any force, there ensues almost immediate relief—so marked that I have ventured, at the risk of being tedious, to recount the method and meaning of an apparently very simple process.

The cases in which every ordinary method of replacement fails to prevent recurrence of flexion were just those in which trial was made of the stalked pessary of Sir J. Y. Simpson. When this was condemned as hazardous, and the reasons for its falling into disfavour were understood, there remained no other course than to leave these patients to their sufferings, condemned to dysmenorrhœa, sexually inapt and sterile, and unable to make any physical effort or endure the least exertion. I was painfully impressed as to the extent of hidden misery which women have to endure, often inexplicable to themselves, and more frequently of such kind that they do not choose to talk about it, by a case where the patient cheerfully professed her readiness to submit to wearing a stalked pessary, even after full explanation of the professional dicta as to the serious risks involved.

It was with full recognition of the difficulties and hazards attending the introduction of a foreign body within the uterus, so as to exercise mechanical influence on its position, that the instrument now to be described was devised. I consider it essential for an efficient intra-uterine pessary that it should not interfere with the movements of the organ itself, since these must go on. I recognised that only mischief could result from allowing the possibility of pressure against any one point of the intra-uterine surface; that any continued dilatation of the cervix would certainly induce expulsive action, and that the only way in which physical elasticity could be safely counteracted was by elastic resistance.



This instrument for assuring the continuance in place of a flexed uterus resembles in appearance a short sound, having a guard-plate (A) at such distance from the extremity as to represent nearly the normal uterine length. After replacement of the uterus, the instrument is introduced just as an ordinary sound until the guard comes to touch against the cervix, when the smoothly-rounded end (B) lies free within the cavity of the uterus, the intervening part being bent to the natural curve of the organ. The finger retains the guard-plate *in situ*, whilst the canula (C, D, Figs. 1 and 2) is withdrawn. The spring pessary expands, as shown in Figs. 2 and 3, lying along each lateral wall of the intra uterine cavity. The breadth of the spring prevents any antero-posterior flexion, and its elasticity antagonizes expulsive effort, whilst the movements of the uterus, as a whole, are in no way restricted. The withdrawal of the instrument is accomplished by introducing the canula until its point touches the hollow on the vaginal surface of the guard-plate. There is a slit on the back of the canula which receives the metal bar connecting the spring with the guard-plate. Thence it readily glides into the cavity, and collapses the spring, as in Fig 1. There is no need to employ the speculum either for introduction or withdrawal. It was my original intent that the springs should be plated with gold; but I find that the steel undergoes but slight superficial change, without any erosion of its smooth surface, even when retained for a fortnight. This is the longest time it has been continuously worn; but in this case it caused no inconvenience, and was only removed on account of impending menstruation.



This instrument, like all local appliances, whether mechanical, physical, or chemical, is but ancillary to, and may not be allowed to supersede, the more important general therapeutical treatment required in all uterine complaints, whether these be lesion of structure, deviation of place, or disorder of function. The success of any new plan of treatment can only be fairly judged after the lapse of a very considerable period, and when the patient has for some time resumed the accustomed routine of a healthy woman's life. I purpose in a further communication recording the results of my own experience of the use of the instrument, of which I have here described the mechanism and purpose.—*Lancet, Feb.* 24, 1866, *p.* 198.

92.—A NEW UTERINE DILATOR.

By J. W. Hembrough, Resident Accoucheur at St. Bartholomew's Hospital.

[The uterine dilators at present in use are deficient in their action upon the internal os and upper part of the cervix.]

The accompanying engraving represents the instrument I have had constructed, the advantages of which will, I think, be sufficiently obvious to those who are acquainted with the dilatation now in general use.

Whether dilatation is required for the introduction of a metrotome, sponge or laminaria tent, or stem, especially when we consider the almost universally strictured condition of the *internal* os in mechanical dysmenorrhœa, it is obvious that the *internal* os and upper part of the canal require to be dilated to at least an equal extent with the remainder.

The engraving represents the instrument open. It has three joints, a central (B) and two lateral (A and C). To the central one (B) is attached a rod (D), which is included within the two halves when the instrument is closed. By shortening the rod D, dilatation is accomplished to the extent required by turning the screw handle.—*Lancet, March* 3, 1866, *p.* 240.

93.—ON VAGINAL RECTOCELE.

By James R. Lane, Esq., Surgeon to St. Mary's and the Lock Hospitals, and to St. Mark's Hospital for Diseases of the Rectum.

It occasionally happens, when the perineum has been partially ruptured, or when, without any rupture, the perineum and vaginal outlet have been so much dilated, or have become so much relaxed, as not to afford an adequate support to the pelvic viscera, that dilatation of the rectum and posterior wall of the vagina is gradually induced, and these parts are protruded through the vulva in the form of a tumour, which is at first of small size, but which, if measures are not taken for its relief, may become in course of time as large as, or larger than, the fist. To this condition the term *vaginal retocele* was applied by M. Malgaigne, in a paper published in 1838, in which he has described it in his usual complete and exhaustic manner. It had, however, been previously noticed by Sabatier, who pointed out the distinction between simple prolapse of the vagina, and the prolapse of the rectum with the vagina now under consideration. It was also well known to Sir Charles Clarke, who, in 1814, gave a clear and accurate description of it under the name of *procidentia vaginæ*, while, somewhat inconsistently, he called the analogous condition in which the bladder and anterior wall of the vagina are protruded, *procidentia vesicæ*. Such a nomenclature could not fail to cause confusion. It is much better, therefore, to adopt the terms proposed by Malgaigne, of vaginal rectocele for the one case, and vaginal cystocele for the other, and to restrict the term prolapse of the vagina to the descent of the vaginal membrane only, not involving the adjacent structures. This latter affection, though described by various surgical writers, is, I believe, of very rare occurrence, setting aside, of course, those cases in which it is associated with the descent of the uterus. Malgaigne, who closely investigated the subject, could only meet with three cases of *true* prolapse of the vagina, and in all three it was partial and confined to the posterior part, which was protruded to a limited extent, without any corresponding dilatation of the rectum. I know of no authentic instance of prolapse of the anterior wall without corresponding descent of the bladder; but then it must be remembered that the cellular connexions between the bladder and vagina are much more dense than those between the vagina and rectum. Indeed the vagina is so closely connected with the urethra and neck of the bladder, that its separate protrusion at this part can scarcely be imagined possible.

It should be understood that in vaginal rectocele there is no displacement of the entire rectum; its posterior surface maintains its normal relation with the sacrum and coccyx; its anterior surface only is involved, and becomes dilated into a sort of pouch, which carries the vagina before it as a covering. When fæcal matters descend into the rectum, they distend this pouch, and the muscular fibres, being weakened by the dilatation, are unable to expel them; while the abdominal muscles, when called in to assist, instead of acting so as to overcome the resistance of the sphincter, expend their force upon the pouch, which they tend still further to dilate.

A rectocele is, in most cases—indeed in all which have come under my notice,—of gradual formation. Malgaigne, however, mentions two instances in which it was suddenly produced—in one case by a fall, in the other by lifting a heavy weight. Both these patients were in an advanced stage of pregnancy.

On examination of the parts, protrusion of the posterior wall of the vagina is observed; and if the patient strains, the protrusion will be seen to increase in size. On introducing the finger into the rectum, it can be readily turned forwards into the swelling, thus showing that the rectum as well as the vagina is involved; while, by passing the finger into the vagina in front of the swelling, the uterus is usually felt to be in its natural position, or nearly so.

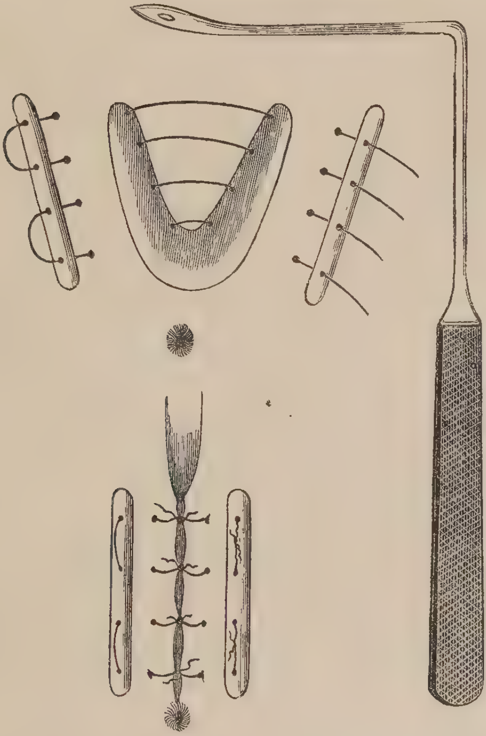
The affection is one which causes great discomfort and distress: there is a feeling of weakness, of weight, and bearing down in the perineal region, with obstinate constipation and great difficulty in evacuating the rectum, which sometimes cannot be effected until pressure has been made upon the tumour to squeeze the contents out of it—until, in fact, the deficient contractile power of the rectum has been artificially supplied, and properly directed towards the natural outlet. Leucorrhœal discharge is a very frequent accompaniment.

I have stated that partial rupture of the perineum predisposes to this affection. It is otherwise, however, when the rupture is complete and extends through the sphincter. In this latter case there is incontinence instead of constipation; for when the fæces pass into the rectum there is nothing to prevent their escape, and dilatation therefore can hardly occur. I should have thought the existence of rectocele altogether incompatible with *complete* rupture of the perineum, but for a case recently recorded by Mr. Bryant, in which the two conditions were present in the sâme person. In this individual probably the dilated rectum had been present before the laceration of the perineum took place. Out of eight cases of rectocele which have come under my notice, there was partial rupture of the perineum in five; in two there was no evidence of rupture, but the perineum was short and the parts relaxed; in one only was the perineum more than an inch in length.

It is necessary to remark that prolapse of the posterior wall of the vagina might possibly proceed from other causes than that now under consideration. A vaginal hernia, for instance, in which the intestine descends between the vagina and rectum, pushing before it the recto-vaginal fold of peritoneum, might cause a tumour similar in external appearance. Sir Charles Clarke mentions a case in which protrusion of the vagina was caused by the descent of cysts connected with diseased ovaries. It might also possibly be dependent upon an abscess in the recto-vaginal septum. The nature of the case, however, is at once made clear by examination of the interior of the rectum, the cavity of which will be more or less encroached upon in any of the cases just mentioned; whereas in rectocele it will be found to be dilated, and the finger can be at once passed forward into the tumour.

It is only within the last few years that any effectual surgical remedy has been devised for this complaint. The measures formerly recommended were merely palliative in their extent. Sir C. Clarke recommended the evacuation of the rectum by enemata, the use of astringent injections, and, in severe cases, a globular pessary to be worn in the vagina. Malgaigne relates cases in which attempts were made to diminish the calibre of the vagina by dissecting away portions of the mucous membrane covering the swelling, and uniting the edges of the incision by suture; but the result was not encouraging, and he therefore was content with a pessary of a peculiar description to retain the swelling in its place. The removal of portions of the apparently redundant vaginal membrane is not likely to be of much service: it does not remedy the corresponding dilatation of the rectum, and it does not supply the want of support in the pelvic floor which is the essential cause of the complaint. The only effectual mode in which that support can be supplied is by closure of the ruptured or relaxed perineum to a sufficient extent to retain the protrusion; and of late the operation for suture of the perineum has been applied by several surgeons (in the first instance, I believe, by Mr. Baker Brown) to the cure of rectocele, with very satisfactory results. I have myself practised it repeatedly, and it has in every instance fully answered my expectations. Indeed, whatever doubt there may be as to the efficiency of the operation in producing a permanent cure of prolapse of the uterus or of vaginal cystocele, I believe there need be none as to its effect, both immediate and remote, in vaginal rectocele.

The following is the plan of operation which I have adopted for some years past for suture of the perineum, whether for rectocele or for rupture of that part. The principle is that of the quill suture, but in several of its details it will, I venture to think, be found an improvement on the methods in ordinary use. The patient being placed in the lithotomy position, a portion of skin and mucous membrane is dissected off on each side of the lower half of the vulva, so as to form a raw surface, which should be about an inch and a half in length on each side, the right and left portions being continuous with each other below across the median line. It should be an inch or more in depth antero-posteriorly at the lower part next the anus, but may diminish to about half an inch in depth towards its upper part. It is better first to mark the outline of this raw surface by incision with the scalpel, and then to dissect off the mucous membrane, the thinnest possible layer of which should be removed; but it should be taken away in one piece, and not in small fragments. I much prefer this plan to transfixing the part with the knife, and cutting a sort of flap from within outwards: in the latter way a larger amount of tissue is removed, which is objectionable, and vessels of larger size are likely to be wounded. By proceeding in the way above described I have never had occasion to place a ligature on a bleeding vessel, nor have I ever met with bleeding sufficient to cause inconvenience, either at the time of the operation or subsequently. Care should be taken that the denuded surface is not situated too far outwards upon the buttock or too far inwards towards the vagina, but just where the opposite sides would naturally and readily come in contact. The deep sutures, which are to hold the quills, are next to be inserted. For this purpose I am in the habit of using a strong needle of rectangular shape, set in a handle, and with an eye near the point, as represented in the figure. This should be entered unarmed, on the left side (the terms right and left refer to the patient, not to the operator), a full inch external to the anterior border of the cut surface; it should be passed deeply to take hold of as much tissue as possible, and brought out close to the posterior edge of the raw surface. It should then be thrust onwards through the tissues on the right side at a corresponding depth, and made to penetrate the skin at a point corresponding to that of its entry on the left. The eye near its point is now threaded with the wire suture, and the needle is withdrawn carrying the suture with it. The needle should be bent at a right angle about three inches and a half from the point, and should be slightly curved from the angle to the point. Such an instrument can be passed with much greater readiness than the needle in ordinary use, in which the handle is in a line with the shaft. I have found the latter difficult to pass, on account of the necessity of depressing the handle against the left buttock in order to carry the point through the integument of the opposite side. This inconvenience is entirely avoided with the rectangular needle, in using which also the surgeon's hand is in a much more convenient position, and the needle is passed through the parts with a simple turn of the wrist.



I am in the habit of using four deep sutures of silver wire, and of fastening them to perforated ivory bars, which represent the quills. Each ivory bar is about the size of a quill, and is perforated with four holes about half an inch apart, but they should be of different lengths for different cases. The ivory bar intended for the right side is ready threaded with two pieces of wire as represented, each piece of wire being looped through the two adjacent holes. This is held by an assistant, and the needle, after it has been passed across, is threaded with one of the four ends of wire and withdrawn ; the proceeding being, of course, repeated with each of the other three. The wires are then passed through the holes of the second ivory bar, and being drawn tight, the whole is firmly secured by twisting together the ends, first of the two lower, and then of the two upper wires. By having the wires looped on the one side, no fastening is required on that side; while on the other side the two adjacent sutures are fastened simultaneously. Thus not only is time saved, but a uniform pressure upon the part is more easily secured. A single silver wire of sufficient strength to hold the quill suture securely is somewhat stiff and unmanageable. I have therefore latterly used very fine wire, twisted in three strands in the form of a rope. This gives equal or greater strength, and is at the same time much more flexible and convenient in use. Metallic sutures are greatly to be preferred to those of thread or silk. The latter, from their absorbent properties, produce much more irritation and suppuration along their track, and are therefore less favourable to immediate union.

The quill suture serves to hold the deeper part of the raw surfaces in contact, but the cutaneous edges must also be held together by four or five superficial sutures of fine silver wire. These may be most conveniently inserted by means of Dr. Simpson's tubular needle. The operation, as above described, may readily be completed in ten minutes or even less. In a case of this kind the sphincter is entire, and its action does not in any way interfere with the apposition of the cut surfaces; it is therefore quite unnecessary to divide it.

The patient is then placed in bed on her side. Sufficient opium is given to prevent the action of the bowels for the first six or seven days, and the urine is drawn off twice or thrice daily with a catheter. The deep sutures should be cut and removed, together with the ivory clamps, at the end of forty-eight hours. Some œdematous swelling generally takes place, but soon subsides after the pressure of the quill suture has been removed. I have never seen any benefit from leaving the deep sutures longer than this ; on the contrary, whenever I have done so, I have found that a good deal of additional irritation, and often suppuration, has been set up, without any corresponding advantage. When metallic sutures are employed, there is rarely any suppuration along their track at the end of the second day, or if there is any, it is very trifling, and never in my experience to any injurious extent. The superficial sutures need not be removed till the sixth or seventh day, at the end of which time tolerably firm union will in most instances have been obtained. During this period a nutritious but unstimulating diet should be given, without restricting the patient's inclination as to quantity, and with a moderate allowance of beer or wine; but stimulants in any quantity, as sometimes recommended, I believe to be unnecessary in ordinary cases. The bowels should now be emptied by a brisk aperient, and the opiates discontinued. The catheter also may be dispensed with. In three or four days more, the patient may generally be allowed to move about, and at the end of a fortnight those operated on in hospital are often able to return to their homes. At the end of three weeks there is usually complete cicatrization throughout, the part is soft and flexible, and all tenderness consequent on the operation has disappeared.

The result of this operation for rectocele is in the highest degree satisfactory. I have practised it in seven cases, and in all of them have obtained a thick and firm perineum, capable of effectually retaining the protrusion. The sensation of weakness and bearing down is no longer felt, and the constipation, so far as it depends upon the mechanical difficulty in emptying the rectum, is relieved. It may be objected, that the restoration of the perineum does not cure the dilatation of the rectum and vagina, but merely prevents their protrusion externally. I believe, however, that it does more than this, and that when the parts are retained in their proper place, they will gradually return to their proper dimensions. It must be remembered that the dilatation is not the essence of the complaint, but only takes place for mechanical reasons, in consequence of the loss of support in the pelvic floor. When that support is again supplied, the dilatation by degrees disappears, or at any rate is so much lessened as to be of no practical inconvenience. In several cases which I have had opportunities of examining after the lapse of a year or more from the operation, I have been able to find scarcely any remains of the pouch which was formerly so evident. The protrusion is not likely to be reproduced, for the stress falls upon the posterior part of the new perineum, where it is thickest and strongest, and if the operation is properly done, there is no chance of this giving way.—*Lancet, Feb.* 3, 1866, *p.* 117.

94.—A CASE OF OVARIOTOMY IN WHICH THE PEDICLE WAS TIED AND RETURNED, AND THE LIGATURE REMOVED IN FORTY-EIGHT HOURS.

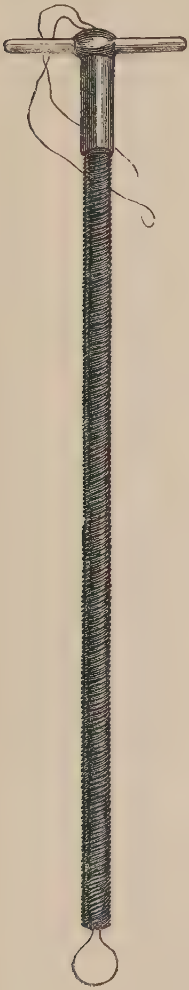
By Dr. J. H. Aveling, Senior Medical Officer to the Sheffield Hospital for Women.

Mrs. A. W., aged 45, keeps a small shop and mangle, came into Sheffield Hospital for Women, suffering from an ovarian tumour. She has been married twice, and has two children. The eldest is now twenty-five, and the youngest twenty-two. Her second marriage took place in February, 1864. Her general appearance and complexion are good. There are, however, a few small boils on the abdomen, and slight œdema of the ankles.

The measurements are—1. Girth at the umbilical level, forty-one inches. 2. From ensiform cartilage to umbilicus, nine inches. 3. From umbilicus to symphysis pubis, nine inches. 4. From right anterior superior spine of ilium to umbilicus, nine and a half inches. 5. From left ditto to umbilicus, nine inches.

There is too much distension to make out whether adhesions exist or not. Fluctuation is very distinct. The uterus is pushed over to the left side, and can only be moved slightly. The length of its cavity is three and a half inches. The catamenia appears every three weeks, but the discharge is small in quantity. It was entirely suppressed for three months in 1864. Finding herself getting stouter, the patient then thought she might be pregnant: but as it hurt "her belly to lift," she concluded that something must be wrong, and sent for her medical man, Mr. Parker, who diagnosed ovarian tumour. From that time to the present she has continued to increase in size. Her breathing is very laboured, and accompanied by a troublesome cough. She sleeps on her right side, but is most comfortable when sitting nearly upright.

August 3rd, 1865, with full consent of the patient, the operation was performed, Drs. Jackson and Keeling and Messrs. Gillott and Sharp were present, and rendered valuable assistance. An incision was made between the umbilicus and the pubis, five inches long, commencing an inch below the umbilicus. As soon as the cyst was reached it was tapped, and the fluid withdrawn without passing the hand into the abdomen. No adhesions existing, the sac was drawn out easily. The pedicle, which was short and about four inches broad, was transfixed with a needle carrying a double thread. The two ends of each ligature were then brought up together on either side, and drawn through two iron-wire coils. Each coil had fitted upon its end a cross-bar, and round these cross-bars the ligatures were secured after they had been drawn sufficiently tight to compress the vessels. The pedicle was then placed in the hook of my polyptrite and crushed through. As there was no hemorrhage it was returned at once *in situ*, and the wound closed with iron wire sutures. The heads of the two coil-clamps were left protruding from the lower angle of the incision.



The tumour consisted of five cysts, communicating with one another, and of a colloid mass about the size of the placenta. The cysts contained twenty-one pints of fluid.

August 4th. Patient comfortable. Has taken food well. Pulse 84. No abdominal tenderness.

5th. Has pain caused by flatulence. Pulse 80. The ligatures were unwrapped from the cross-bars, the coils removed, and the ligatures drawn out.

6th. The wound is healed, with the exception of a small aperture the size of a quill, through which the coils passed. The sutures were removed, and plaster put on instead. Pulse 85.

8th. Has had a feverish attack, followed by an eruption which extends over the forehead, arms, chest, and abdomen. She is restless and uneasy. Pulse 95.

10th. Eruption fading. Pulse 80. The slight discharge which has continually escaped from the small opening has ceased.

From this time the patient had not an unfavourable symptom, and, after two or three weeks of good living, she was discharged cured.

October 23rd. The patient walked to the hospital this morning, a distance of two miles, and says she is able to attend to her shop, and, now and then, to take a turn at the mangle.

The coil-clamp, which is figured in the preceding sketch, will probably be found most useful in cases where the pedicle is short, as by its use all dragging may be avoided. In the foregoing case two clamps were used. One would probably have been quite sufficient. They may be obtained from Messrs. Hutchinson, Duke-street, Sheffield.

Mr. Spencer Wells said that no one method of dealing with the pedicle could be applicable in all cases of ovariotomy. A long pedicle was dealt with so successfully by the clamp that he desired no better method; but it was still doubtful what was the best mode of proceeding when the pedicle is short. Tying it and cutting off close to the ligature, and returning it with the ligatures, also gave excellent results in persons of good condition. But in feeble persons, where copious serous effusion might be expected, and it appeared desirable to maintain an opening for drainage, the plan of the earlier ovariotomists, of leaving the ends of silk or twine ligatures hanging out through the wound, was so unsuccessful that the apparatus of Dr. Aveling would probably prove of great advantage in cases of short pedicle in weak patients. Ovariotomy was an operation still in its infancy, and anything which seemed likely to improve any of its steps was worthy of investigation and trial.

Dr. Marion Sims said that the clamp was a great improvement on the ligature. He thought Mr. Baker Brown's plan of severing the pedicle by the actual cautery promised great results. He viewed Dr. Aveling's method with favour. He always used a silver wire, and left it to be sacculated. He said the distal end of the severed pedicle did not slough where the wire was applied. The wire became embedded and hidden in its tissue. He saw this in a case in which he had applied the wire for M. Nelaton, whose patient died five days after the operation. His countryman, Dr. Peaslee, had proven that the stump of the pedicle did not slough even when tied with a common ligature, for he had made three *post obit* examinations, in all of which this fact was verified. He thought the plan of dealing with the pedicle was still in a transition state; and he hailed with pleasure every improvement in this direction, for from this he expected a still greater reduction in the mortality of ovariotomy.—*Obstetrical Transactions, Vol.* vii, 1866, *p.* 229.

95.—ON CHLORATE OF POTASS IN THE TREATMENT OF OVARIAN DISEASE.

By W. Craig, Esq., Ayr.

Disease and enlargement of the ovarium have long resisted all kinds of treatment, and many medicines have been used in attempts to absorb the tumour, but hitherto unavailingly. Extirpation for some time past has been the favourite method of treatment, but it is attended with a large expenditure of life,

—at an average, nearly every second case. It is very desirable, then, that a mode of treatment could be discovered which would prevent such an expenditure of life, and this usually when the patient is in the most hopeful and interesting period of her existence.

In the following cases the cure has been effected by the administration of chlorate of potass. I could form no opinion regarding the nature of the tumours, other than that they were ovarian.

In his work on the Science and Art of Surgery, Mr. Erichsen states, that "medical means exercise no influence in curing, and but little if any in retarding the progress of ovarian tumour."

Dr. Charles Clay, of St. Mary's Hospital, Manchester, makes the following statement in the London Medical Review :—"I conscientiously believe that neither medical treatment, external or internal applications, pressure, nor galvanism, are of the slightest benefit. They neither cure nor palliate the disease. All such attempts, then, are fallacious, and only throw obstacles in the way of any benefit that extirpation of the tumour offers ; increasing the difficulties of that operation, if not defeating it altogether."

The treatment in the following cases is an exception to the above statement, as they were treated and cured by means of a very simple medicine, viz :—Chlorate of potass. It may be that one only of a species into which the disease is divided may be of a nature to be removed by this medicine. and, consequently, the others may remain uninfluenced by it ; but, during the life of the patient unless after the operation, it is not always possible to learn the exact nature of the tumour ; but whatever be its character, it can do no harm to the patient to allow her to have the benefit of a trial of this medicine, as it has a favourable action on the functions of the body, irrespective of the action on the tumour.

Chlorate of potass, as is well known, is a medicine that can be used with the utmost freedom. I do not pretend to offer an opinion as to its *modus operandi* in this disease. The circumstance of this salt having in combination a large quantity of oxygen, which is held feebly by the potass, and is left free in the system, may cause it to operate beneficially on the constitution, invigorating and improving the animal functions so as to enable them to throw off these morbid growths.

Case 1.—Miss S., of Ayr, is of middle height, sallow complexion, and apparently of sound constitution. The tumour is on the left side, rising out of the iliac region. It is about the size of a child's head of a month old. The patient states that the tumour is sore when pressed, also during defecation and micturition. She menstruates regularly. It is free in its attachments and rolls from side to side as the patient turns in bed. It is about five years since she first observed the enlargement, and it was about a year after this when she first applied for medical advice. She had the counsel of many medical men, and took many medicines, but received benefit from none of them. She was under the treatment of one medical man during the twelve months immediately before coming to me. This gentleman used many medicines and numerous external applications, all without any beneficial effect. He then with the consent of other medical men, resolved to perform the operation, but he died when preparing for it.

When she came to me I immediately put her upon a saturated solution of chlorate of potass,—a dessert-spoonful thrice daily. She stated that she had only taken the medicine two or three weeks when she felt a gradual improvement in her general health. The tumour gradually diminished in bulk till, at the end of ten or twelve months, it disappeared. After the tumour had been so far reduced as not to be felt through the parietes of the abdomen, it could be felt in its greatly reduced size lying close to the uterus. About this time it was about one and a-half inch in diameter. Subsequently, the tumour has disappeared completely, with the uneasiness and symptoms depending on its bulk, and she has since continued in her usual health.

Case 2.—Miss C., from London. The attention of this lady was first called to her complaint when taking a bath in June 1861. At this time she felt a swelling rising from the right side of the pelvis. It was then about the size of an egg, and moved from the side to the middle of the abdomen. There was no pain in the tumour when the body was at rest, but in quick walking and some other forms of bodily exertion, it seemed as if bound by a light network all over the lower part of the right side. There was frequently a dull pain in the iliac region, and more rarely a sharp, stinging pain; but when at rest, or in ordinary walking, there was no pain. The patient states that her health was very sensibly affected by it. "I lost strength and tone, and became listless." The tumour grew rapidly from June to September, but after this the enlargement was slow in its progress. The tumour till now was always movable, but subsequently it became more fixed.

She came expressly to Scotland to consult an eminent practitioner, and was under his treatment two months of the summer of 1862, and nearly as long in the same season of 1863.

It was in the autumn of 1863 that she consulted me. On examining the tumour I noted no particular induration of its texture, but its size was about that of a large fist. Her former adviser had used many medicines and appliances with but little effect. The patient was twice cupped and leeched over the tumour, and the skin was twice painted over with a preparation which acted like a blister, and was also painted many times with combinations of iodine. She had tonics from the beginning, and daily a solution of bromide of potass. She also passed an electric current through the tumour for half-an-hour daily, and this was done during two years. The only effect of the treatment hitherto applied was to make the tumour "more compressed or harder." Immediately on her application to me I commenced the administration of the saturated solution of chlorate of potass in dessert-spoonfuls thrice daily, and with what effect I shall allow the patient to tell. "I will add, that about three or four months after I had seen you, and taken the medicine you prescribed, the swelling disappeared as it came, silently and suddenly. I continued your medicine, and the occasional use of the electric battery until a few months ago. I use neither now; and as I said before, I have no swelling; none whatever; none."

I saw this lady in London a short time ago, and I could not discover a vestige of the tumour.

Case 3.—Miss H., from Glasgow, has had for a considerable time a tumour on the left side in the left iliac region. It was about the size of a large fist closed. She had used the chlorate of potass for two or three months before observing much diminution of the tumour, and at this time left off the use of medicine. She was induced, however, to commence the use of the medicine again, and she states, that during the last few weeks, whilst using a renewed supply, the tumour has become much smaller, and gives less uneasiness.

A fourth case, in Ayr, presented herself with a small tumour in the left iliac region. The tumour had been observed for many months. It was painful, more especially when it was pressed. The size could not be well estimated, as the abdominal parietes were thick. I commenced with the chlorate of potass, but the patient soon became impatient and would not persevere, and has failed to continue the medicine.

So few cases go but a small way to establish the efficacy of this medicine in the cure of ovarian tumours; but, in view of the declaration of experienced and practical men who have seen much of this disease, and who maintain that medical means exercise no influence in curing, and but little in retarding the progress of ovarian tumours, it is right to produce facts, in however small numbers, when they show that medicine is not so inefficacious as represented by some writers on this subject.

It may be admitted that before publishing my experience of the efficacy of chlorate of potass in the treatment of ovarian disease, I ought to have been able to present a larger number of successful cases; but in a small town, amongst a rural population, there is less chance of meeting a satisfactory number of cases than in the crowds of large cities.

Though the number of examples be small, the success attending the treatment ought to encourage a trial of a medicine that can be used with the greatest freedom, and could always be tried—provided there is no contra-indication—before having recourse to such a formidable operation as that of ovariotomy.—*Edinburgh Med. Journal, Nov.* 1865, *p.* 427.

96.—ON THE TREATMENT OF OVARIAN TUMOURS.

By I. Baker Brown, Esq.

On numerous occasions I have secured adhesions especially to the omentum, by silver wire, cutting off the ends.

On September 9th, 1862, I secured a mesenteric artery, which was bleeding freely during operation, by silver wire, the patient recovering.

Since the introduction of Mr. Charles Clay's adhesion clamp, I have divided all adhesions and arrested all hemorrhage by actual cautery, and now divide the pedicle by the same method. The success attending this practice will, I believe, cause it to supersede all other methods of securing the pedicle in ovariotomy.—*Medical Circular, June* 14, 1865, *p.* 398.

97.—ON IODINE IN THE TREATMENT OF UTERINE LEUCORRHŒA.

The treatment of leucorrhœa is a constant subject of difficulty and vexation to the medical practitioner. Although the use of various astringents will often effect improvement, yet this is seldom lasting, and the recurrence of the symptoms is a continual source of annoyance. We have lately observed a plan which is being pursued by Dr. Murray at the *Great Northern Hospital*, and which promises to be a very useful addition to our means of treatment in this very troublesome condition. Dr. Murray first ascertains, by means of the speculum, that the discharge proceeds from within the uterus. He then introduces a small short-haired brush (much like that used for washing phials) by a screw-like motion, so that the thick phlegm-like layer on the uterine wall is swept off with every turn of the brush. When this reaches the fundus he steadily withdraws it, charged as it is with the mucous deposit. Its place is then taken by a gum-elastic catheter with several apertures, through which is injected a lotion consisting of one part of the compound tincture of iodine to two parts of water. The uterine wall is thoroughly washed with this. The muscular contraction which follows this injection is remarkable, the tube being tightly grasped, so that its reintroduction at the time is extremely difficult. Dr. Murray has reason, after an experience of many cases treated by this plan, to feel highly satisfied with its success.—*Lancet, Jan.* 6, 1866, *p.* 7.

98.—ON DYSMENORRHŒA, METRORRHAGIA, OVARITIS, AND STERILITY, DEPENDING UPON A PECULIAR FORMATION OF THE CERVIX UTERI; AND THE TREATMENT BY DILATATION OR DIVISION.

By Dr. ROBERT BARNES, Obstetric Physician, and Lecturer on Midwifery and the Diseases of Women and Children, St. Thomas's Hospital; Examiner in Midwifery, Royal College of Physicians; &c.

[The opinions have been various as to the existence of, and the nature of narrowing of the cervix uteri: some think the contraction exists most frequently or always at the os internum; others at the os externum.]

The condition that prevails most frequently seems obvious enough. If a woman has suffered long from painful menstruation and metrorrhagia, and has been married some years without pregnancy, it may be predicated with great confidence that the neck of the uterus projects in an abnormal degree into the vagina; that its form is conical; that the os internum is a small round orifice, barely admitting the uterine sound; and that probably there is deviation of the cervix backwards or forwards, or to one side. This form of cervix has been aptly described by Ricord as the "*col tapiroïde.*" It is, no doubt, congenital, and may be traced back to imperfect development. It is sometimes associated with imperfect development of the body of the uterus and of the ovaries; but this association is certainly not a necessary one. In the majority of instances the ovaries perform their function, irregularly, it is true; and pregnancy frequently occurs after the error of formation of the cervix is corrected.

Another circumstance to be noted is the rather frequent association of this peculiar form of cervix with a short vagina and general defective development of the pelvic organs.

The vaginal portion may project into the vagina from half an inch to one inch, an inch and a half, or even two inches. This excessive projection into the vagina may be due to a preternatural hypertrophic elongation of the lower part of the cervix; but I believe it is commonly due to the reflection of the fundus vaginæ taking place at a higher point of the cervix than usual.

In the ordinary construction the cervical canal communicates freely with the vagina by an open transverse fissure, inclining, indeed, to a circular form in the virgin. The form of the cervical cavity is thus a flattened cone or funnel, of which the base is open. The vaginal portion projects as a flattened semisphere scarcely half an inch into vagina, the vagina being reflected off from the cervix a little above the level of the os externum. In some cases the vaginal portion is a cone, the apex projecting nearly an inch in the vagina, and the vagina being reflected off nearly on a level with the os uteri internum. Instead of the natural free communication between the cervical cavity and the vagina, the os externum is so contracted as to form a sensible obstruction. Indeed, sometimes the ordinary uterine sound can be passed only with difficulty; and I have known the occlusion to be complete at times, requiring some little force to break down a thin membranous septum formed at the orifice. As soon as the os externum uteri is penetrated by the sound, it is usually found that the point enters into a sufficiently capacious cervical cavity. This cavity, in fact, is exactly spindle-shaped; it narrows again towards the os uteri internum, which is, as Dr. Henry Bennet has shown, naturally a small opening. In cases of protracted suffering from the dysmenorrhœa attending the peculiar form of cervix under consideration, I have, however, generally found that the sound passes through the os internum without difficulty. It is by this observation that I have come to the conclusion, already mentioned, that in some cases the excessive projection of the vaginal portion is simply due to the vagina being reflected off from the cervix at a higher level than usual. In such cases the introduction of the sound to the fundus uteri shows the entire length of the organ to be normal.

The seat of the obstruction, then, I believe to be almost invariably at the os uteri externum. The obstruction is due chiefly to the small round os itself; partly to the pointed elongated form of the lower part of the vaginal portion, and partly to an unusual rigidity of structure of this part, which impedes the expanding action natural to the healthily formed os uteri.

With this coarctation of the os externum uteri, and undue projection of the cervix into the vagina, we might almost reason out most of the consequences which observation has detected. Some of them I will briefly describe. The narrowing of the vaginal orifice of the womb will, as has been often described, cause obstructive dysmenorrhœa. This is, indeed, sometimes so marked as to resemble retention of the menstrual fluid from complete occlusion. There is a partial occlusion, and the same symptoms, only less in severity, follow. The ovarian excitation of menstruation causes engorgement of the uterine mucous membrane; blood is effused; it cannot escape readily, and whilst fluid; it tends to accumulate in the uterine cavity and in the cervical cavity, and sometimes to form coagula. The blood so gathering and pent up in the uterus causes efforts at expulsion, betrayed in spasms or uterine colic. These pains are constantly likened to labour-pains; they recur at distinct intervals of from five to fifteen minutes, are extremely severe, and are commonly partially relieved on the discharge of a quantity of blood or coagula.

This is often the character of menstruation in girls having this formation, from the very establishment of the function; but sometimes menstruation only assumes this painful character after the lapse of some time.

The next consequence is periodical congestion beyond the normal degree of the ovaries and uterus. Preternatural congestion seeks relief in hemorrhage. The mucous membrane of the Fallopian tubes and uterus pours out blood copiously. Hence we have metrorrhagia or menorrhagia, in addition to dysmenorrhœa. The patient is distressed by the recurrence of the menstrual flow after intervals of two or three weeks only, and by its lasting for a week or ten days.

A secondary consequence—I am persuaded not an infrequent one—is an oozing of blood from the abdominal end of the Fallopian tubes and from the surface of the congested ovaries. This is a form of retrò-uterine hæmatocele of which I have seen several instances in young girls. Symptoms of shock announce the outpouring of blood into the peritoneum, or in the cellular tissue of the broad ligaments; intense pain in the abdomen and pelvis, fever, announce the reaction and peritonitis. As in almost every instance of retro or peri-uterine hæmatocele, a simultaneous escape of blood takes place externally.

Attendant upon repeated congestions, ovaritis may follow, entailing plastic adhesions between the Fallopian tubes and ovaries, and limited pelvic peritonitis. These adhesions may become a cause of sterility, and will, I believe, sometimes induce atrophy of the ovaries.

All these results may occur in single women. When women so suffering marry, the symptoms set forth are often much aggravated in severity, and new evils add to their distress.

1. *Sterility* is an almost constant condition. Occasionally one meets with examples of the conical cervix with small os with the history of a childbirth some years previously, and more frequently with an account of having suffered abortion. Strict inquiry will generally throw doubt upon the reality of the abortion. The profuse flooding and discharge of clots interpreted by the patient as evidence of abortion are often nothing more

than the metrorrhagia characteristic of the uterine obstruction. Where impregnation takes place, it usually does so in young persons early after marriage, that is, before the further disorders, which we shall presently notice, have had time to be developed.

2. *Cervicitis interna et externa* and *endometritis* are amongst the first consequences of marriage. The periodical engorgements of menstruation easily pass into subacute and chronic inflammation, chiefly affecting the mucous membrane lining the cervical cavity and the vaginal portion. The secretions resulting from this inflammatory action find difficult exit from the cervical canal. Hence there is increased, often constant, pain. Conjugal relations become almost intolerable. It is common now to find a highly vascular state of the cervical mucous membrane; the small portion seen just inside the minute os is deep-red, angry looking. There is loss of epithelium round the os. The introduction of the sound easily causes bleeding. At first the discharge is chiefly albuminous; afterwards it becomes muco-purulent and even sanguineous, and by its abundance entails serious constitutional debility.

Not infrequently, vaginitis follows, chiefly affecting the upper part of the canal. This I have several times found so intense as to produce vaginismus, and render sexual relations impossible.

3. Whether or not the inflammation described be produced, *deviations of the projecting vaginal-portion* are very common. The most ordinary form is flexion or curving of the cervix forwards. Sometimes it is driven backwards. Lateral deviation to the right or left is often observed. In fact, the excess of vaginal portion is equivalent to a foreign body in the vagina. It is thus constantly liable to injury in the shape of inflammation and displacement.

I believe flexions of the cervix upon the body occur far more frequently in this way than as the result of a fibroid body growing in the wall of the body of the uterus, as represented by Dr. Marion Sims. The deviation really belongs to the cervix, and not to the body. Whenever deviation takes place the prospect of impregnation is still further impaired. The minute os externum is thrown aside, and there is stricture by angulation at the seat of the os uteri internum.

I have notes of a case which shows in a striking manner the severity of the symptoms sometimes produced. A young lady had been married two years without pregnancy. Since marriage she had suffered from metrorrhagia, and several attacks considered to be, and treated as, peritonitis. During the last six months she has had a constant sense of swelling with pain in the left ovarian region; vomiting attends the pain.

This has been relieved by leeching. When I saw her, metrorrhagia had continued six weeks without cessation. Great prostration was present, with irritative fever, reminding me of pyæmic puerperal fever. I found a small conical cervix, with an os so minute that it required considerable pressure to introduce the uterine sound; deviation of the cervix to the left; defined tumefaction and pain in the left ovarian region. I inferred that the constricted os externum, impeding the flow of blood from the uterus, led to the formation of coagula in the cavity; that these coagula were broken up by decomposition; that absorption of septic matter took place, causing constitutional symptoms, and possibly cellulitis in the left broad ligament; that the tumefaction in the left broad ligament might also be due to hemorrhagic effusion. I split the cervix with my instrument. The metrorrhagia, which had persisted for six weeks, and was abundant at the moment of the operation, ceased in a few days, and the pain abated.

The treatment of these cases has given rise to much discussion. It is convenient to divide the subject under two heads:

(A) The general treatment of the effects of the mechanical obstruction.

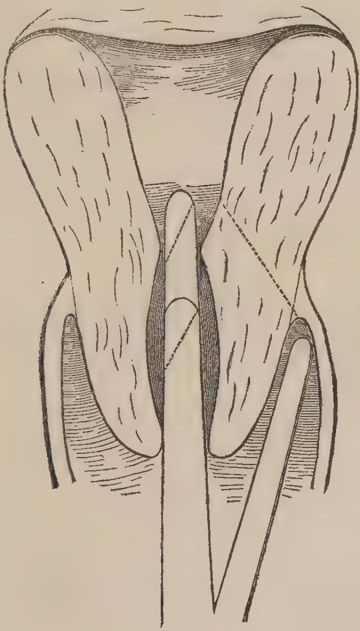
(B The direct treatment adapted to remove the cause.

(A) Depending, in great part at least, upon a mechanical cause, any plan of action that is not adapted to remove this is not likely to do more than mitigate the symptoms. One class of remedies will be called for to control or moderate the hemorrhage, and to counteract the consequent anæmia. A considerable amount of success may attend the use of nitric and hydrochloric acids, cinchona, iron, Indian hemp, Chian turpentine, cinnamon, digitalis, and the other styptics and tonics in general use. I have not seen much benefit from bromide of potassium. The pain and colics will demand special remedies; opium, belladonna, Indian hemp, are often serviceable. Special treatment will also be necessary to relieve the local inflammations. Vaginitis and aggravated cases of cervicitis externa require, above all things, *rest*, in the fullest acceptation of the term—rest, functional as well as physical. To attain this end, repose in the horizontal position is important. Possibly this, aided by the application of pledgets of cotton-wool soaked in glycerine and tannin, by injections of various astringents or of plain water, by occasional cauterization with nitrate of silver, will suffice. But in more severe cases, where a large surface of vaginal mucous membrane is inflamed, spasmodic contraction of the muscular coat is apt to follow. The effect of this is very distressing. The rugæ of the vagina are preternaturally enlarged; the contractions of the muscular coat cause chafing of the opposed prominent rugæ; hence abrasions of epithelium, purulent discharge, sometimes blood, and the most intense pain. The essential condition of recovery from this state is *rest*. This I accomplish by getting the patient to wear a vaginal support, made of india rubber, of a somewhat cylindrical form, which is inflated with air after introduction. I have not found the division of the sphincter, recommended by Dr. Marion Sims, to be necessary. The form of vaginismus to which I am referring is, indeed, different in origin and nature from the cases described by Dr. Sims ; the vaginismus here is strictly the result of inflammation. The action of the vaginal rest I employ is, first, to keep the irritable and inflamed walls of the vagina apart ; the prominent rugæ, no longer chafing against each other, have an opportunity of getting well. Secondly, the easy mechanical support afforded to the walls of the vagina quickly subdues the morbid contractility of the muscular tissue. Very soon the inflammation and irritability subside simultaneously. By this means I have cured several very aggravated cases. In one instance—that of a lady who had been married sixteen years without being able to endure conjugal approaches, and in whom an examination was not possible without the administration of chloroform—complete relief was afforded, ending in pregnancy and the birth of a living child. The application of this principle and means of procuring local rest is much more extensive. I resort to it in cases of extreme engorgement and inflammation of the cervix, following upon labour.

It remains to consider the treatment as directed against the cause of the disorders under discussion. The importance of this treatment must be admitted when we reflect upon the severity and danger of the disorders that call for relief, and the hopelessness of giving more than partial and temporary relief by means directed simply against the consequences of a physical obstruction.

The principal means of overcoming the coarctation of the os externum uteri are dilatation and incision. It would be superfluous to enter at any length on the description of the numerous contrivances that have been devised for dilating the cervix ; but it is of great moment to determine the amount of safety and efficaciousness which attend dilatation. If it were proved to be more safe and efficacious than incision, then it ought to be preferred. Evidence is conflicting. I will not venture to offer an opinion as to the relative value of the facts and arguments adduced by others. I cannot, however, estimate lightly the circumstance that men of great experience, sagacity, and resources, like Simpson and Marion Sims, have abandoned dilatation in favour of incision. They have found dilatation neither safe nor efficacious. Hemorrhage, pyæmia, cellulitis, peritonitis, have undoubtedly followed dilatation ; and it is certain that in many cases, however good the dilatation effected by bougies or tents may appear at first, it is not of long duration. I suppose there is no dilatation by instruments more powerful than that effected by pregnancy and labour, yet even after giving passage to a full-grown child the peculiar cervix under consideration will sometimes completely resume its old vicious form. The sea-tangle-tents lately come into use certainly possess advantages not before attained, and so strengthen the case for dilatation. But, admitting that dilatation can be completely attained, are all the indications, all the requirements of the case, answered? Will dilatation remove the congestion, the inflammation, the deviations which attend the conical cervix? Here I believe dilatation fails, and incision, or splitting the cervix, is an operation possessing a marked superiority in results.

Fig. 1.



I will briefly advert to the instruments employed. There is, first, the well-know *bistouri caché*, or metrotome of Professor Simpson. In fig. 1, is represented this instrument in action. It is well adapted to answer its purpose of dividing the cervix, but requires great care in use. After introduction into the cervix, the blade is detached from its guard in such a manner as to open at once to the full width at which it may be set by the screw in the handle. The incision it makes, if not very carefully controlled by the hand, will be liable to extend deep into the substance of the cervix at its upper part, near the junction with the body of the uterus. There is no doubt that incisions so made have actually gone through the substance of the cervix, and wounded the plexus of vessels outside.

Incisions so free in extent, and involving the higher portion of the cervix—its most vascular part—may be expected to cause considerable hemorrhage. This is a great objection—one all the more serious in that such free incision is altogether unnecessary. It will be seen that objection lies partly against the rationale of the operation, and partly against the instrument.

Another instrument has been devised on the same principle as Dr. Simpson's, but having two blades, which diverge and cut both sides of the cervix simultaneously. There is no advantage in cutting both sides at once; on the contrary, the operator having to use an instrument constructed to do its work like a machine, is deprived of the opportunity of guiding and moderating his incision. The single-blade of Dr. Simpson is less open to this objection. It may be made to work according to the judgment and skill of the operator.

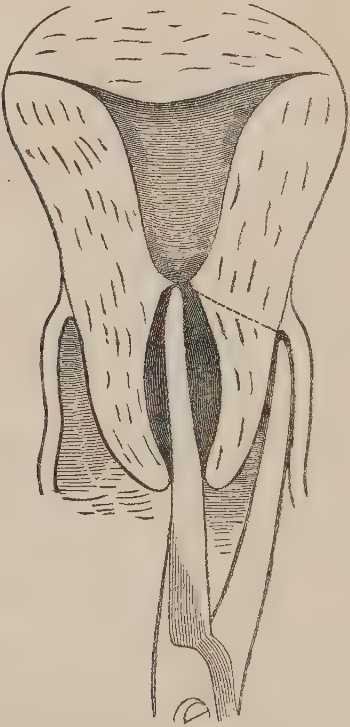
Dr. Greenhalgh has contrived an ingenious modification of the double-bladed metrotome (described 'Obstetrical Transactions,' Vol. V.) The blades do not diverge to the full width at once, but gradually, forming an incision of the form of an ellipsis. The incision being narrow at the upper part avoids to some extent the danger of cutting deeply into the substance of the cervix. The division of the projecting part of the cervix made by it does not extend to the angle of reflection of the vagina. I used this instrument for some time. Owing to the extreme fineness of the blades, the incisions made are so clean as to favour hemorrhage; and unless great care is taken to keep the lips of the wound apart by tents, its reunion is very apt to take place. It is, moreover, open to the objection stated against the old two-bladed metrotome, namely, that it is too much of an automatic machine; not enough of a surgical instrument.

Feeling this objection, and that an instrument should be absolutely governed by the intelligence of the operator, and not so made as to work after its own fashion, I contrived an instrument to cut like a pair of scissors, which I think is free from all the objections applying to the instruments or machines just described. One blade, strong and somewhat probe-shaped, passes through the narrow os into the cervix, whilst the other blade, slightly concave on the cutting edge, so as better to seize the rounded surface of the cervix, is applied at any point of the vaginal-portion the operator chooses. Then, the part to be divided being exactly included between the blades, is cut through. Thus we are sure of cutting as much as we want, and no more. There is this great advantage in cutting from without inwards instead of from within outwards, that *we are cutting towards safety and not towards danger;* we cannot cut deep into the substance of the cervix on a level with the os internum; the incision partakes of the crushing character. These two circumstances afford better security against severe hemorrhage or other danger. When one side of the cervix is divided, the other is treated in like manner. The instrument is most simple, inexpensive, and not likely to get out of order. The wound made by it is less liable to reunite by first intention. It *gapes* better than that made by the knives. With this instrument, made for me by Messrs. Weiss a year ago, I have operated many times at the London Hospital and in private and consultation practices. Its action has almost always been entirely satisfactory. It produces a good os tincæ. (The instrument is represented in action, fig. 2.)

It afforded me sincere gratification to see in the Lancet of the 1st of April last, a paper by Dr. Marion Sims, in which it appeared that he had for some years before adopted the same principle of proceeding as myself, and was in the habit of using a similar instrument. Our distinguished *confrère*, however, fixes the cervix by means of a vulsellum before cutting. I have not found this to be necessary. My experience entirely corroborates that of Dr. Sims as to the safety and efficaciousness of this operation.

I will conclude with a brief *résumé* of the advantages attending division of the cervix, as I have performed it, over dilatation. I believe that dilatation is preferred by some physicians who have seen or heard of severe hemorrhage after division by the single or double knife, on the ground that it is safer, and entails less responsibility. I doubt whether dilatation be safer than division as performed by Dr. Marion Sims and myself. I have never seen hemorrhage at all alarming; it is usually stopped in five or ten minutes by applying pledgets of cotton-wool soaked in cold water; and it is rare to find any secondary bleeding of moment. To guard against this, and at the same time to obviate the tendency of the wound to unite, I press a bit of oiled lint between the lips of the wound, and lightly plug the vagina with oiled lint to keep the dressing *in situ*. I need hardly say that rest in bed for two or three days is enforced.

FIG. 2.



Any troublesome bleeding may be quickly arrested by iced water, by perchloride of iron, or by firm plugging.

As to the permanency of the new os tincæ I am not able to speak with certainty. There is no doubt a tendency to contract; but I question whether the contraction ever proceeds to the extent of reducing the os to its original minuteness; nor is the risk of contraction so great as after simple dilatation. It may be necessary to repeat the operation after the lapse of some years; but I believe this will rarely happen.

Mr. **Baker Brown** differed from the author as to the seat of stricture; he believed it to be in the cervix itself, generally accompanied by narrowing, contortions, and reflections of the canal, the results of inflammation. He thought stricture either at the internal or external os, was exceptional. Mr. Brown described his mode of operating, which was as follows. Having placed the patient in the lithotomy position, he introduced a bent speculum, and then grasping the anterior lip of the os uteri with a pair of vulsellum forceps and holding it up in the median line, he introduced Simpson's hysterotome, first having arranged the blade so as to determine the depth of the incision to be made. He cut from within outwards completely through the os externum and neck, as far as possible, without wounding the vagina; having done this on the right side, he reversed the instrument and did the same on the left. He had no fear of hemorrhage when the subsequent steps which he adopted were fully carried out, namely—inserting oiled lint well into the cut surfaces, and then plugging the whole vagina with lint or cotton-wool, so as to prevent any admission of air which migh loosen the pledgets of lint in the wound. Mr. Brown had always found Dr. Simpson's instrument to answer exceedingly well; and had tried, but did not like the double hysterotome. He, however, thought it highly important that the operator should see what he was doing, and therefore objected to Dr. Simpson's method of operating, with the patient on the side, and using the finger as the only guide to the os uteri.

Dr. **Greenhalgh** stated that he had listened with great attention to the president's paper, and was extremely surprised to hear that he considered the seat of stricture in cases of dysmenorrhœa to be at the external os uteri, and that the neck of the uterus was small and tapering. A large experience had convinced him (Dr. G.) that although such cases are occasionally met with, still in the great majority the seat of stricture will be found at the internal os uteri, the neck of the uterus being somewhat enlarged, and not tapering; consequently he deemed it essential that the internal as well as the external os uteri should be divided. The drawings sent round were evidently ill developed virgin uteri in which the operation is rarely or never required. After division, Dr. Greenhalgh introduced his bilateral expanding stem which keeps up steady dilatation and prevents contraction of the parts. Much had been said about hemorrhage, but he could positively affirm that out of nearly one hundred cases of division of the os and cervix with his metrotome, in one case only was plugging had recourse to, and in this instance the patient was extremely plethoric; moreover, the uterus was much congested, and she was the subject of severe pruritus, and on the eve of a menstrual period; the operation was perfectly successful. Dr. Greenhalgh considered the great advantages of his method were the extreme exactitude, quickness, and almost painlessness with which the incisions can be made, and that without any exposure of the patients during or after the operation.

Dr. ROUTH fully confirmed Dr. Greenhalgh's view. For his own part, he believed in by far the majority of cases the obstruction was at the *inner* and not the *outer* os; although he did not deny that in some cases of conoid cervix it was present at the external os. He agreed with Dr. Gream in believing that Dr. Sims's plan of operation would occasionally leave a deformed cervix for life; and he did not think it was necessary to cut through the entire cervix. The instrument Dr. Greenhalgh had invented obviated all danger from hemorrhage. The same was true of his (Dr. Routh's) instrument, which he, however, preferred, because of the bend, and, therefore, more easy of application in *flexion* cases. A little bleeding was salutary. In most of these cases there was a complication of congestion, which the very incision by the subsequent hemorrhage relieved. But there was no doubt that such incisions, however freely made, had a tendency to contract again. Hence it was necessary to keep the cut made patent by some internal uterine pessary, and for some time, it might be for months, so as to allow it to become properly lined with mucous membrane and incontractible. He knew several persons now walking about London with these. In other cases their removal had been followed by conjugal relations and pregnancy, though previously sterile for years. Of the use of sponge-tents and other modes of artificial dilatation, in these cases, he spoke disparagingly. He had seen cellular abscess and death follow their use. They should be used with the greatest caution. He also believed cases of dysmenorrhœa were more common than was generally supposed. Not only was the seat of obstruction more frequently at the *internal* os than the *external*, but, indeed, in many cases the external os was patent and abnormally so, as shown by Dr. Henry Bennet. And there were many, and by far more numerous cases of dysmenorrhœa which were in no way due to stricture at either os. As these cases were not, however referred to by Dr. Barnes, he did not allude to them further.

[Dr. SAVAGE laid before the Society an injected preparation of a virgin uterus.]

He believed the preparation to be an excellent specimen of a uterus which had never been impregnated. It appeared to him to warrant the following deductions:

1. Except in cases of irregular vascular distribution, which happen far from seldom, the vaginal portion of the cervix could be divided without risk of serious hemorrhage, division beyond this would endanger the lower cervical arteries.

2. An incision laterally deeper than $\frac{1}{8}$th of an inch at any part of the canal of the cervix or isthmus would be unsafe.

3. An internal os uteri surrounded by a ring of muscle (attributed to Kölliker) as asserted by Mr. Wells, does not exist. The isthmus is surrounded by muscular fibres subjacent to the mucous membrane ; but the complete identification, so to speak, of the latter with the rest of the uterus, and the extremely oblique decussations of the former could not contribute to the formation of a so-called inner os. The uterine isthmus could never form a stricture.

4. That the instruments so vaunted for the cervical dilatation of the cervix most fortunately for operator as well as patient, did little else than divide half through the vaginal portion of the cervix, and this even not efficiently. The complete division of the vaginal part of the os required the scalpel or scissors.

5. That the least bending of the isthmus, where only a bending could occur, would cause an obstruction, without the least constriction or narrowing organically, of that part of the uterine canal, such obstruction not being remediable by cutting.

Dr. Savage said experience amply confirmed these deductions. The easier introduction of a sound through the isthmus after surgical dilatation, as ordinarily performed, would be no proof that the so-called triangular incision, commencing with the division of a constricted isthmus, had been made, mere division of the vaginal portion of the cervix was constantly followed by relaxation of the isthmus, whereby could be easily introduced sounds of a size not before admissible.

He believed that obstruction at the isthmus was generally caused by uterine curvature. The canal of the cervix beyond the vaginal portion was never contracted. Obviously a very slight bending would cause obstruction, owing to the antero-posterior coaptation of the corresponding sides of the isthmus. He quite agreed with Dr. Sims, that no straight instrument would relieve an obstruction thus produced. Dr. Greenhalgh's hysterotome was the best in the automatic list of such instruments, but it was half an inch too short, could not be trusted to cut the vaginal part of the os completely through, and could not be made to pass a curvature ; but suppose the uterus was straight, the passage of any such instruments through the isthmus showed that division was not required.

Dr. Sims' denunciation of mechanical dilatation of the uterine cervix and isthmus, Dr. Savage could not comprehend ; Dr.

Gream, a physician of immense experience, had strongly testified to its safety. He (Dr. Savage) was under the mark in stating he had employed it at the Samaritan Hospital, and elsewhere, many hundreds of times, and in this respect would amply confirm Dr. Gream's assertions, besides being quite as safe, to say the least, it was quite as efficacious as surgical dilatation, especially when performed by the new instrumental inventions. Dr. Sims' process certainly left nothing undone ; but he (Dr. Savage) had for years been in the habit of dealing with the worst of such cases as Dr. Sims had described nearly every week at the Samaritan Hospital, and never had failed in overcoming such obstructions with the sponge tent. It might fairly and properly be urged against surgical dilatation performed in any of its various ways—it was useless to do too little, very dangerous to do too much, and just enough left the uterus permanently mutilated. The operators were not the best authorities, as, unknown to them, their failures marched off to other practitioners. Dr. Gream, and indeed every man of note in this department, speak of case after case rendered worse by an indifferent operation, or exposed to all the dangers of abortion after an efficient one. Dr. Sims speaks of furious hemorrhage occurring at the first stage, and the fatal cases are notorious and far from few.

Dr. Marion Sims agreed with the President (Dr. Barnes), and with Mr. Baker Brown, and Dr. Savage, in saying that in the great majority of cases requiring surgical interference the canal was found to be narrowed in the lower portion.—*Obstetrical Transactions*, vol. vii., 1866, *p.* 120.

99.—ON IMPROVEMENT OF THE BLOOD IN CHLOROSIS, AND ON THE ELIMINATION OF CERTAIN POISONS.

By Dr. C. E. Brown-Sequard, F.R.S., &c.

It has been shown that chlorosis is a nervous affection, and that the deficiency of iron in the blood of chlorotic patients occurs only after this affection has existed some time, and is an effect instead of a cause. It has been further shown that iron serves to cure that affection only on account of a peculiar influence which causes the food to be transformed more easily into blood, and perhaps, also, of a direct improvement in the nutrition of the nervous centres. Other metals, which are not normal constituents of blood-globules, especially manganese and silver, (particularly the oxides of these metals,) sometimes act better than iron in cases of chlorosis and anæmia. He who will read attentively the remarkable papers of Bouchardat on the pugilist's training, and on the rapidly curative effect of exercise and good food in diabetes, will soon be convinced of the immense value of purely hygienic means in improving nutrition and secretion. It is quite natural, therefore, that gradually the idea is being abandoned that whenever a principle is diminished in quantity in the blood or in the tissues, the best treatment consists in trying to increase its amount by giving as a remedy the deficient substance. The selection of a remedy as a tonic or a stimulant ought, therefore, to be quite independent of any such chemical view, and we ought to let a good diet and all the principal rules of hygiene take care of the composition of the blood, remembering, however, that it may be useful to the patient to employ the kinds of food which we know to contain, in easily absorbable forms, iron, phosphorus, or some other substance which seems to be in insufficient quantity in the blood or in the tissues.

As regards the elimination of mineral poisons which have produced functional nervous affections, I will only say at present that the discovery of Melsens, which shows the value of large doses of the iodide of potassium for the expulsion of lead and mercury from the system, has been universally recognised. It is important to remember that the dose of that salt must be very large for its successful influence. The statements of Melsens and N. Guillot on this point have received positive confirmation from researches by Dr. W. Budd of Bristol and Prof. Easton, of Glasgow. My own observations agree with the conclusions of these able physicians. The elimination of lead and mercury takes place chiefly by the urinary, salivary, and cutaneous secretions. Consequently saliva must be spit and the skin very frequently washed, so as to avoid a partial absorption of these poisons after they have been secreted.

I will not speak of the elimination of morbid poisons (such as those of diphtheria, rheumatism, gout, syphilis, leprosy, &c.) having produced nervous affections, because nothing of great importance has been found recently that applies to all, or almost all, those poisons. The only fact worth mentioning is, that the iodide of potassium may serve for the elimination of all but one of them.—*Lancet*, *March* 10, 1866, *p.* 247.

100.—GLANDULAR TUMOUR OF MAMMA, WITH CYSTS; INJECTION WITH COMPOUND TINCTURE OF IODINE.

By William Martin Coates, Esq., Surgeon to the Salisbury Infirmary.

[The patient was 36 years of age and unmarried. The left breast was much larger than the right and was tender. In the centre of the affected portion, which measured two inches and a half in its diameters, there was to the touch a feeling of elasticity giving an impression of deep-seated confined fluid.]

On introducing a grooved needle at the softest point a clear fluid escaped.

On the 13th of August I passed the perforated needle of Wood's syringe, with piston down, into the elastic part of the tumour, and by withdrawing it (the piston, see Art. 102, p. 318) removed five drachms of clear fluid, and threw in two drachms of compound tincture of iodine. Very little immediate pain was caused by this proceeding. On the 14th there was some little tenderness and increase of size. These effects soon subsided, and in the course of a month the mamma was not larger than the right, all hardness having disappeared.

On December 25th, 1864, I was consulted by the same lady for another cyst, situated directly below the nipple of the same breast, with the same increase of glandular structure around. I applied the same remedy, with the same happy result; and to-day (Dec. 19th, 1865) I carefully examined the part, and found it free from enlargement, hardness, tenderness, or abnormal elasticity. There occurs at each menstrual period a sensation of fulness and tenderness, subsiding as the period passes away.

The interesting points in the foregoing case are, the bloodlessness, simplicity, painlessness, and rapidity of cure of the cysts, when compared with removal of the tumour, or the laying it open and stuffing it with lint, the treatment recommended by the most eminent authorities. Secondly, the subsidence of the glandular hypertrophy a few weeks after each injection, leading to the inference that the iodine acts in this disease in the same manner as in cases of hydrocele, in which I have frequently seen the enlargement of the testicle, so common in this disease, gradually subside after the treatment by injection of the tincture of iodine.

Wood's syringe enables us to treat cases in which iodine injections are useful in a much less painful manner than by the hydrocele trocar; and if the needle has a rather larger bore it forms an excellent mode of exploring for fluid, as, by withdrawing the piston, considerable traction is exerted upon the liquid contents of a tumour.—*Lancet, March* 3, 1866, *p.* 226.

101.—CURE OF A LARGE RANULA BY INJECTION OF COMPOUND TINCTURE OF IODINE.

By William Martin Coates, Esq., Surgeon to the Salisbury Infirmary.

[The patient was 10 years of age. The ranula was large.]

June 11th, 1865. I injected from within the mouth, by means of Wood's syringe, fifteen minims of compound tincture of iodine into the tumour. On withdrawing the needle, a drop of the fluid peculiar to ranulæ oozed from the puncture.

12th. A little hardening of the swelling, but no pain.

21st. Tumour rather smaller. On this day I injected thirty minims of the tincture from without by means of Wood's syringe.

22nd. There is considerable hardening of the ranula, and some tenderness on handling it.

From the last note the tumour daily diminished, and on the 15th of July it could not be either seen or felt.

I cannot find by reference to those works of surgery in my possession that the cure of ranula has been attempted by the injection of iodine. I therefore have assumed that the idea is an original one of mine. It is so simple, so painless, so free from danger and annoyance, that I have ventured to place it among my interesting cases. It is true that one case is too small a basis on which to found a treatment; but as there must be a first case, and as ranula (according to my experience) is not a common affection, I publish it in the hope that others may give it a trial; and perhaps in the future, those suffering from this disease may escape, through my means, incision, excision, the cautery, the seton, the opening it in its whole length and stuffing it with lint, or the platina tubes of Dupuytren,—all of which must result more or less in failure, pain, bleeding, or distressing salivation.—*Lancet*, *March* 3, 1866, *p*. 226.

102.—ON A CASE OF SPINA BIFIDA CURED BY INJECTION OF A SOLUTION OF IODIDE OF POTASSIUM.

By W. Martin Coates, Esq., Surgeon to the Salisbury Infirmary.

[The tumour was about the size of a large walnut, transparent and flaccid, and was saturated at the lower part of the last lumbar vertebra and the upper part of the sacrum.]

The opening into the spinal column was about half an inch long and a quarter wide. It was easily felt. The child was otherwise well formed and healthy. There was no paralysis of the lower extremities. When the child cried or struggled the tumour became very tense. I gave a desponding prognosis. The tumour progressively enlarged, becoming daily less flaccid when at rest, and more tense during exertion or crying.

About a month after the birth of the child, I read, in an extract from the Boston Medical Journal, that "two American physicians, Drs. Brainard and Crawford, had treated seven cases of spina bifida by iodine injections, and that five were cured, one dying seven months afterwards of chronic hydrocephalus. They used from a quarter of a grain to four grains of iodine, and from three-quarters of a grain to twelve grains of iodide of potassium, dissolved in from one drachm to several ounces of water. The injections were repeated as often as necessary, the strength of the solution being gradually increased. The punctures were made in sound skin at the side of the tumour. No more of the fluid was withdrawn than was replaced by the injected solution. After the operation collodion was applied, and for months after the tumour had disappeared."

I proposed to operate on Rosa L. A consultation with Mr. James Paget was decided on. He entirely coincided with me in recommending the operation, as without it the case was hopeless. The infant, on its return to Salisbury, caught whooping-cough ; in consequence of which the operation had to be put off.

On the 8th of January, 1860, the tumour was so tense, and the covering had become so thin at one point, that I determined to lose no more time. I operated in the following manner, chloroform being very cautiously administered by my partner, Dr. Blackmore :—Instead of an ordinary trocar, I used Wood's syringe, holding half a drachm, with a pointed perforated needle. Having screwed on the needle, and pressed home the piston, I thrust the needle through a healthy part of the skin at the side of the tumour into its cavity, drew out, by withdrawing the piston, half a drachm of clear fluid, unscrewed the needle, and emptied the syringe; pressed the piston home, screwed it on again, and again withdrew half a drachm of fluid, and so on until I had taken out two drachms. The tumour was then very flaccid. I injected, by reversing the process, two drachms of the following fluid :—Of iodine, ten grains ; of iodide of potassium, twenty grains ; of distilled water, one ounce. The tumour was painted over at once, and daily afterwards, with contractile collodion. The thumbs of the patient were immediately convulsively drawn into the palms, and the toes of both feet pointed. The infant looked very ill, and refused the breast during twenty-four hours. She then rallied, took the breast, and from that hour had no unfavourable symptoms. At the time of the operation the covering of the tumour was so thin that the colour of the solution of iodine was very apparent through it.

From this time the cure proceeded uninterruptedly. The tumour became flatter, its covering grew first mottled by deposition of white matter in its substance, then evidently, though gradually, more dense, until when quiet it (the tumour) did not project more than an eighth of an inch from the surrounding skin, and all its covering, except about the size of a pea, became thick and opaque. The child is, and has been since two months after the operation, well, and runs about as vigorously as other children of her age.

As will be seen by the perusal of the above, the original idea did not emanate from me. The idea of curing spina bifida by the injection of a solution of iodine originated with Drs. Brainard and Crawford, American physicians. The modification of the operation I introduced was the using Wood's syringe and needle instead of the small trocar. By this, my plan, a very small puncture is made; there is much less chance of wounding nervous tissue; and so small a puncture, if care be taken, could not admit atmospheric air. The puncture healed immediately.

I used chloroform, not only to save the child pain, but to prevent shock, recollecting the experiments described by Dr. Claude Bernard, in which it was found impossible to destroy the spinal cord in frogs without killing the animals, until chloroform was given them, and then they survived the experiment.

The peculiar convulsive action bringing the thumbs into the palms, and causing pointing of the toes, is interesting, as being very similar to the effects of centric or eccentric irritation of the spine.

The case of Rosa M. L., is strongly favourable to the view that spina bifida is essentially a local dropsy of the arachnoid membrane of the spinal cord, and that the fluid, carrying with it the membranes of the cord, either prevents ossification at the point of its exit, or causes absorption of the tender bony covering of the spine by continued and increasing pressure.

If this hypothesis be accepted, the alterative action of the iodine on the lining membrane of the cord is explained, and it would account for the failure of puncture, incision, or excision in these cases. By the injection the disease itself is attacked; by the simple puncture, &c., the effect of the disease is attempted to be removed, but the diseased action is neither altered nor controlled, and danger is run of arachnitis of the cord.

I believe this is the first case related of the kind by an European operator. The injection of a strong solution of iodide of potassium and iodine into a sac communicating directly with the spinal cavity seems indeed a bold proceeding; but, according to my experience, there is a marvellous tolerance of iodine in the animal tissues. Besides, this disease seems otherwise beyond our art.

The iodine injection appears to do in these cases that which nature does in rare instances. It arrests the abnormal secretion of serum. That being done, the covering of the tumour contracts and thickens, so as to protect the membranes of the spine at that point from external injury.

Another mode of radical cure by an effort of nature is by adhesion of the arachnoid at the opening in the spinal column; but it is evident that the abnormal and progressive secretion of serum must first have been arrested, otherwise death must have ensued by pressure on the spinal cord and brain. In such cases of isolation of the swelling, it may be as safely removed as an ordinary tumour, and this, I believe, has been done. It is, however, a hazardous proceeding, as with thickened integuments it is impossible to ascertain that there is not some slight communication with the membranes of the spinal cord, and then arachnitis of the cord is very probable.—*Lancet, March* 3, 1866, *p.* 225.

103.—PRACTICAL REMARKS ON THE LOOSENING OF THE ARTICULATIONS OF THE PELVIS.

The morbid relaxation of the articulations of the pelvis is but the exaggeration of a physiological fact, the object of which is to facilitate the passage of the fœtus. When the ligaments are much relaxed, at the close of pregnancy, the patient is often entirely unable to take walking exercise, and the circumstance may give rise to errors of diagnosis. It may, therefore, not be uninteresting to advert to M. Trousseau's opinions on the subject, such as they are represented in the Union Médicale by M. Peter, formerly the Clinical clerk of the learned Professor at the Hôtel Dieu.

Before offering any remarks on two cases observed in his wards, M. Trousseau briefly related the history of two ladies in whom he detected for the first time the symptoms of this condition of the pelvis.

At the age of 36, Mrs. X. was delivered of her third child. The accouchement had been in every way regular, but through motives of precaution, on which it is unnecessary to dwell, M. Trousseau required her to remain in bed for an entire fortnight, and to lie on a couch for an equal period. She was then permitted to rise, but when she attempted to stand she experienced a severe pain in the back and pelvis, and for more than a month was unable to walk round the room. Slight leucorrhœa being present the Professor ascribed the symptoms to a trifling amount of metritis, a frequent consequence of parturition; he, therefore, merely prescribed a few injections.

One day, however, the patient having endeavoured to walk in his presence, he was much struck by her attitude ; she *waddled*, dragging each leg with difficulty, and bending to the right or left as she advanced one or the other foot. She found it impossible to stand on one leg, and if she endeavoured to do so she fell, and complained of severe pain in the back and hips. Mrs. X.'s extraordinary debility suggested the idea of an affection of the spinal cord, but, on a careful investigation of the state of common sensation and mobility, it was ascertained that the integument was in full possession of all its functions, and that every movement was performed with ease provided the patient was in the horizontal attitude. It then occurred to M. Trousseau that the symptoms might arise from relaxation of the articulations of the pelvis.

Pressure over the pubes and the sacro-iliac joints occasioned considerable pain. The Professor at once bound the pelvis as tightly as possible in a circular bandage, and the patient, to her great delight and surprise, was immediately able to walk firmly, and without pain. A laced jean belt was then constructed, to support the pelvis and hips, and as soon as this bandage was ready she resumed her usual domestic occupations. After an interval of six weeks the use of the belt was discontinued, and a complete cure was effected.

The second patient was a young woman of 25, who presented similar symptoms ; and M. Trousseau, enlightened by his former experience, at once pronounced the case to be one of relaxation of the pelvic articulations, and prescribed a bandage which was worn for two months, and finally consolidated the loosened joints.

At the Hôtel-Dieu, a stout young woman, who had recently been confined, was found incapable of standing while her bed was being made. She was carefully examined ; the uterus was in a perfectly healthy condition, and when lying down she performed with ease every movement of the limbs, common sensation being at the same time preserved in its full integrity. Even in bed, however, the patient complained of pains in the pubic and sacro-iliac regions. When she moved about she complained rather of a distressing sense of weakness than of actual suffering ; she could not stand upright unless her hands had some support ; and if she attempted to walk she dragged her legs, and was compelled to rest after every three or four steps, requiring always the assistance of an arm or the prop of the back of a chair, and thus progressed from bed to bed, grasping the iron curtain-rods. No pain was elicited by pressure over the articulations of the pelvis ; no swelling and no redness were present. But the joints were perceptibly loosened, and the fact was apparent when attempts were made to test the separate mobility of the ossa ilii.

M. Trousseau then showed that in an instant the power of free locomotion might be restored to this woman. A circular bandage was tightly applied around the pelvis, and she at once walked with entire ease. She remained in the wards during the time required for the construction of a jean belt, and she has since entirely recovered her natural vigour and agility.

On the 12th of July, 1864, a strong, healthy young woman of 24, whose labour had been most favourable, was extremely alarmed, on the ninth day after her delivery to find herself utterly unable to stand or walk ; whenever she attempted to do either she experienced, in the lower extremities, a sense of extreme weakness, a pain which she compared to a bar across the pubic region, and also a distressing feeling of weight in the small of the back. These uncomfortable sensations also recurred in bed on any sudden effort to turn round. Trusting that she might recover her strength and that the pain would subside under the influence of rest, she returned to her bed for several days, but when she again ventured to rise found she had made no progress whatever. She then determined on applying for admission into hospital twenty-three days after her accouchement.

On examination, the patient was found unable to stand or walk without great pain, and after every effort of the kind she fell back on her bed. The organs of generation were vaguely indicated by her as the seat of the pain, which in reality existed in the symphysis pubis only ; here a marked interval was detected between the articular surfaces, sufficiently wide to admit the tip of the forefinger, and, in addition, the cartilage was obviously softened. The exploration having proved extremely distressing, no attempt was made to cause the ossa ilii to move separately, the diagnosis being otherwise fully established.

Cases of this description have been brought forward by many authors. MM. Debout, Courot, Moutard-Martin, for instance, have expatiated on the good effects, under similar circumstances, of tightly-secured circular bandages, and M. Dévilliers has demonstrated in the most peremptory manner, that in recent cases a cure may sometimes be effected by entire repose and astringents. Professor Stoltz, who has also adverted to the subject, opines that the affection often yields to rest and to the tonic virtues of the waters of Baden-Baden. This is, however, a question well deserving of the practitioner's most serious attention, because the loosening of the pelvic joints gives rise to symptoms which have too often been deemed indicative of uterine disease and depletion, and antiphlogistic treatment injudiciously resorted to. We should, however, observe that, as a consequence of the puerperal state inflammation may sometimes exist as a complication. In this case, the most carefully constructed bandages are not so efficacious as entire repose ; but even then, puriform arthritis may arise, and death supervene, as in the case of the late lamented Princess Mary of Orléans, who died at Pisa of an affection of this kind.—*Medical Circular, June* 21, 1865, *p.* 403.

104.—ON THE MORTALITY OF CHILDBED AS AFFECTED BY THE NUMBER OF THE LABOUR.

By Dr. J. Matthews Duncan, Lecturer on Midwifery, &c., Edinburgh.

[In this paper the two following questions are considered. Does the number of a woman's pregnancy regulate in any degree the mortality to be expected from lying-in ? Does the age of the child-bearing woman regulate in any degree the mortality accompanying this function ? The following are the conclusions arrived at.]

1. The mortality of first labours is about twice the mortality of all subsequent labours taken together.
2. The mortality from puerperal fever following first labours is about twice the mortality from puerperal fever following all subsequent labours taken together.
3. As the number of a woman's labour increases above nine, the risk of death following labour increases with the number.
4. As the number of a woman's labour increases above nine, the risk of death from puerperal fever following labour increases with the number.
5. If a woman has a large family she escapes extraordinary risk in surviving her first labour, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children.

These laws, although they merely state coincidences, have very important practical bearings, which are too self-evident to require description. They have also important philosophical bearings, which were alluded to in the commencement of this article. The most important, perhaps, of these relate to puerperal fever. These also I shall not enter upon farther than to say, that the attendance of puerperal fever specially on primiparæ, and women who have born large families,—its pretty close correspondence in relative amount to the general mortality of parturition after different pregnancies,—its subjection also to the law of the duration of labour,—do not appear to me to lend support to the views hitherto generally entertained regarding it, and expressed in the words accidental, fever, contagious, epidemic. Another point under this head I shall merely mention. Authors, comparing the mortalities of lying-in-institutions, whether from puerperal fever or from other causes, are frequently found neglecting to begin by ascertaining whether or not they are fit objects of comparison, and under this head, *inter alia*, neglecting to ascertain the comparative amount of primiparity in each institution. It is plain that, unless there be nearly the same comparative amount of primiparity in the institutions, their respective gross mortalities cannot be justly compared with one another.

The well-known protraction of labour in primiparæ may to some appear a sufficient cause of the increased mortality of first child-bearing. But mere prolongation of labour for a few hours cannot, in my opinion, be regarded as any satisfactory explanation of the causation of this increased mortality. In one set of Johnston and Sinclair's cases, the labours of primiparæ are called purely natural, and they are compared with similar purely natural cases in multiparæ; and the mere addition of a few hours to the length of labour in such primiparæ is not a sufficient cause of their mortality being quite as great as that of similar multiparæ. Denman alludes to "a vulgar and pernicious error which makes no distinction between the slowness and the danger of a labour." It would be to fall into this error to explain the increase of mortality merely by increased length of labour.

It must be held as proved, that according as labour increases in length, so the mortality accompanying it increases; and this is true not only of the whole mortality but also of the special mortality from puerperal fever. This law, although it must have weighty bearings on the mortality of primiparæ with their long labours, cannot be regarded as to any great degree throwing light on it; for we find new increments of mortality after the ninth labour, when we have no reason to believe that labour is more prolonged than in labours preceding the ninth, and in which the mortality is less. In other words, we have the number of the labour denoting increase of mortality where there is no evidence of accompanying increase of its duration. The law of duration, then, does not enable us to explain the variations of mortality in different labours.

To completely exclude the influence of the law of duration would be very desirable; but we see no present prospect of doing this, except by processes of reasoning. Without such, it could only be done by comparing a series of labours of different number, but in all which the duration was the same.

It must be remarked that, while the law of duration certainly has important bearings on the data and arguments herein adduced to show the influence of the number of the labour, and while the extent of these bearings is undecided, it is at the same time equally sure that the law of the number of the pregnancy has important bearings on the data and arguments adduced to show the influence of the duration of labour, and the extent of these bearings is undecided. The mutual influence of the data and arguments in these demonstrations must be great, and it remains for future observers to accumulate materials for either showing the amount of these influences or for a separate demonstration of the laws by data which do not intermingle them in their conditions.

It is worth while to remark, that, restricting for a moment our regard to the great mortality of primiparæ as it exceeds that of multiparæ, taken together, we have a set of cases—those of Johnston and Sinclair—where the deaths were from puerperal fever, and in which the average increase of duration of labour in primiparæ above that observed in multiparæ was 4 hours. In multiparæ the average duration was 8 hours; in primiparæ 12 hours. Looking at this increased duration, and the correspondingly increased mortality in primiparæ, with the light thrown on it by tables published by various authors to demonstrate the law of duration, it appears to me that the increase of mortality in primiparæ is greater than these tables appear to give as the increase corresponding to a rise in duration from an 8 hours' labour to a 12 hours' labour.

These various remarks I have made with a view to keeping the demonstration of the influence of the number of labour on childbed mortality in its proper light, to keep it separate from other laws or supposed laws with which it may be confused. I have alluded, with this view, to the causation of the variations of mortality according to the number of the pregnancy. It is no main part of this paper to enter on this subject, but a few words may not be out of place. It would be foolish to imagine that any injurious influence or the reverse could spring from the mere number of the pregnancy. A woman in a first may and often does have as fortunate a delivery as in any other. To ascribe to the number of pregnancy any potency would be to fall into the error of those students of the duration of labour who ascribe great potency to the mere addition of length to a labour. In the case of the law demonstrated in this paper, and in the case also of the law of the duration of labour, it appears to me that the causation of the variations of mortality is to be looked for in the introduction of complications. I here use the word complications in a much wider sense than is generally ascribed to it, wishing it to imply injuries or injurious tendencies far slighter than those ordinarily classed as complications of labour. I have no doubt that all of these, however minute or slight, have their weight in giving proclivity to a fatal termination of the childbed. Puerperal fever may have its root in an otherwise insignificant perineal laceration as well as in a phlebitis or endometritis.

In primiparæ, as labour goes on, complications occur which are not nearly so liable to attack a women in her next subsequent labours. These have their origin in various sources, chiefly in mechanical difficulties, and these often so slight as not to take the case from the category of purely natural, in an arrangement where the labour is alone taken into consideration, to the exclusion of the childbed.

Multiparæ are specially and increasingly liable to complications of a different kind connected with constitutional diseases, and with local infirmities of the uterus.

This introduction of complications forms also the main explanation of the law of the duration of labour. Indeed, in a rough way, it may be held that the statement of duration is a statement of the increase of complications; for it is known that, as labour lengthens out, so complications increase in frequency. Without these complications duration would be of small importance, as the profession has generally held. Their introduction is present evil and the seed of future disasters. Tables have been framed to show the increasing introduction of complications as labour is prolonged, but I only refer to them. They are quite insufficient, so far as I know them, because they are founded only on an enumeration of those of the graver sort. Further, the introduction of complications is not ruled exclusively by the duration of labour. Many are rather connected with precipitate parturition. The complications which probably contribute largely to produce the increased fatality of labours after the ninth are not all included, or capable of inclusion, in any statement of duration, being present before and after the process.—*Edinburgh Medical Journal, Sept.* 1865, *p.* 209.

105.—ON LIEBIG'S FOOD FOR INFANTS AND INVALIDS.

By Dr. Arthur Hill Hassall.

In the preparation of this food, the two principal objects at which Liebig aimed were—first, to produce a food which should resemble human milk in the relative proportions of its heat-giving and flesh-forming constituents; and, second, to reduce it to the state most easy of digestion and assimilation.

It should be clearly understood, however, that the formula given by Liebig, although it furnishes an article having about the same relative composition as human milk, is yet of twice its strength, or, to use the words of Liebig himself, it contains "the double concentration of woman's milk;" and therefore there is reason to believe that in some cases this food will prove too rich for the infant's stomach, and will require dilution.

It appears to me that the great merit of Liebig's preparation consists in the use of malt flour as a constituent of the food: this, from the diastase contained in it, exercises, when the fluid food or soup is properly prepared, a most remarkable influence upon the starch, quickly transforming it into dextrin and sugar, so that, in the course of a few minutes, the food, from being thick and sugarless, becomes comparatively thin and very sweet. That the action of the diastase on the starch is very considerable is amply proved by the following analysis:—

Uncooked Food.

Albuminous matter	9·25	grains per cent.

Dried Cooked Food.

Albuminous matter	15·84	grains per cent.
Fatty matter	8·49	,,
Sugar or glucose...	37·73	,,
Sugar of milk	10·90	,,
Dextrin and starch	27·04	,,
Total	100·00	

It will be observed, by an examination of the above figures, that a very large proportion of the starch has become converted, in the course of the preparation of the food, into sugar.

Correct and ingenious as are the principles upon which this food has been designed, yet the directions given for its preparation are certainly open to considerable improvement. Thus Liebig directs that the malt should be ground in a common coffee-mill and the coarse powder passed through a sieve. This necessitates the subsequent straining of the food—a tedious operation,—in order to remove the bran and remaining particles of husk. And further, that the food should be put upon a "gentle fire" previous to its being finally boiled. Now, a gentle heat may mean almost any temperature nearly up to the boiling point; and, since the action of the diastase is destroyed at about 150° F., the temperature ought never to be allowed to exceed that degree.

I recommend, therefore, that the malt should be well freed from husk and finely ground; that the wheat flour should be lightly baked; and, finally, that a thermometer should be employed in the preparation of the food. Indeed, in some samples recently submitted to me by Messrs. Savory and Moore, I find that the first two points noticed have been attended to, and that they use malt freed from husk and finely ground, and the wheat flour baked.

The effect of baking the wheat flour is to partially cook the starch entering into its composition, so that less heat is required in the preparation of the liquid food. I find that a temperature ranging between 140° and 148° is amply sufficient to effect the complete transformation and solution of the starch corpuscles, and, indeed, to cook the food sufficiently.—*Lancet, July* 29, 1865, *p.* 135.

ADDENDA.

106.—ON THE CITRIC, ACETIC, AND CARBOLIC ACIDS IN CANCER.

By Dr. John Barclay, Banff.

In the beginning of September of last year, a paragraph was pointed out to me in a newspaper setting forth the wonderfully beneficial effects which had attended the use of citric acid in a case of cancer; but I did not think of trying it, until a patient labouring under a large and excessively painful and cancerous tumour of the neck, behind the angle of the jaw, which from its size, situation, and the extent of its attachments, held out no hope of its successful removal by the knife, requested me to allow her to make a trial of the acid, as she said her clergyman had strongly recommended her to do so. I at once assented, thinking it could have little effect either for good or for harm. But when I called in a few days after, I was somewhat surprised to find that since she had applied a lotion composed of a drachm and a half of the acid, dissolved in eight ounces of water, she had had almost no pain in the growth. This I was the more astonished to hear, as nothing of the anodyne class seemed to have afforded the slightest relief from the pain before, with the exception of the hypodermic injection of morphia, and even this, she said, did not remove the pain so effectually as the citric acid lotion; and besides, the relief from the latter was much more permanent. This treatment was continued for some weeks, with the effect that the patient improved considerably in looks, health, and spirits; and to show if this was really due to the change of remedy, I ordered her to discontinue the wash for a week, during which period of omission the pain returned with as great severity as before, compelling the patient to resume the application, which again brought relief along with it. It now occurred to me, remembering the solvent power of acetic acid over cancer cell walls, to try what effect that acid would have when applied to the tumour. By this time the skin had begun to give way, and a sore to develope itself on the surface of the growth. I therefore ordered vinegar to be applied, and was glad to find that this controlled the pain quite as well as the citric acid had done before. Dr. Manson, the Senior Surgeon of Chalmers' Hospital, in this place, who had used the citric acid lotion with equally beneficial results in another case of cancer (this time of the breast, and totally unsuited for excision from its extent, adjacent glandular enlargement, and the length of time it had existed) now changed it for the vinegar application, and he had no reason to be disappointed, for he found that its anodyne power was equal to that of the citric acid, and also that it possessed advantages which the acid formerly used by us possessed in a very much more slight degree. In the case of the wounds in the breast, which were numerous, and all partaking of the characters of cancerous sores in a most marked degree, it was noticed that after the application of the citric acid, the "thick, serrated and everted edges" did not seem quite so thick, serrated and everted as before, but looked thinner, softer, and with less induration around them. But, and as I hoped would turn out to be the case, the application of the acetic acid lotion, gradually increased in strength from that of common vinegar upwards, produced in this way a much more marked effect, for the edges begin to thin down much more rapidly, granulations of a seemingly healthy character arose in the centre of the wound, all fetor disappeared from the discharge, and even attempts at cicatrisation began to take place, sufficient in several of the smaller ulcers to close them in altogether.

The acetic in varying degrees of strength had now been used for a month in both cases, and looking back over this period, and over the month during which the citric acid had been used, to the condition of both patients previous to the use of these remedies, we were of opinion that not only had their state of health, and the appearance of the disease in each, not become worse, but that both had most sensibly improved. Both patients ate and slept much better than before, and were able to go about their usual household occupations with ease and comfort. The tumour in the neck had become decidedly less, there were attempts at a skin-forming process at several points on the edges of the sore, and pain in it was reduced to a minimum. As for the case of the breast, the woman had so much improved in health, that it would have been difficult to recognise in her, the same thin, cachectic-looking creature of three months back, for her cachectic look had wholly disappeared, and no one by looking at her now, could have supposed that she laboured under a disease of such a serious nature, and which had progressed so far. The tumour itself was no larger than it was three months before, perhaps rather smaller, and several of the smaller sores had healed over entirely.

And now it was resolved to try the effect of carbolic acid in the above cases, and this was commenced on December 28th of last year. About this time, two other cases of cancer applied for advice. The one was an extensive tumour of the neck of the uterus, and implicating the whole of the vagina, accompanied by very great pain, and a most profuse and exceedingly fetid discharge. Indeed, so fetid was the discharge, that no one could stay even for a short time in the room with the patient. The other was an enormous schirrous tumour of the breast, of very rapid growth. It had been in existence only four months, and already it extended from the floor of the armpit almost to the sternum. The subject of it had been in one of the largest of our provincial hospitals, and had got nothing, either there or anywhere else, which gave her any relief from the extreme pain, and the horrible fetor of the discharge. The carbolic acid in the form of a very dilute lotion[*] was ordered in all four cases, with the following results.

In the case of the tumour behind the jaw, the lotion was about as effectual in relieving the pain as either the citric or acetic acid lotions. Applied in this weak state, its solvent effect was much the same as that of the citric acid; but applied in a more concentrated form, the effect was a most vigorous eating away of the tumour, and with much greater rapidity than by the two acids formerly used. But there were very feeble attempts at skinning under the use of the dilute carbolic acid in this case, and none of course when the strong acid was employed.

In the case of the mammary tumour, which had been treated before by the citric and acetic acids, the report was that the pain was as effectually controlled by the carbolic acid as by either of the other two; that under its use the "thick, serrated and everted edges" disappeared much more rapidly than with the other two; and that when the weak solution was employed, cicatrisation was seen going on over many of the sores, wherever the cancerous excrescences were eaten down to below the level of the surrounding skin. In the case of the disease of the uterus and vagina, the effect was equally striking; whenever the weak lotion was employed the pain almost entirely disappeared, and with it the horribly offensive discharge. The poor woman, from wishing herself dead, began to have her spirits raised, to eat and sleep well, and now no fetor was perceived by those in the room with her. And the general improvement in her appearance was beginning to be visible when a severe attack of hemorrhage nearly carried her off, since which time her progress towards recovery has been very slight.

A like result was obtained in the fourth case, that of the large mammary tumour. The pain, instantly on the application of the carbolic acid lotion, disappeared as if by magic, and the fetor of the discharge was very much lessened. The tumour was extirpated a few days after the application of the acid, and the case has gone on well since.

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[*] ℞. Acidi carbolici, ʒiss—ʒij.; spiritûs vini rectificati ℥j.; aquæ ad ℔ij.

Two or three more cases of cancer of the breast had the carbolic acid lotion applied, but the patients live at a distance, and no report has been received from them.

I may mention that, on treating cancer-cells under the microscope with acetic and carbolic acids in varying degrees of dilution, I found that in about equal strength the carbolic acid dissolved the cells much more rapidly and effectually than the acetic acid, and caused the nucleus also to disappear almost entirely when applied in a concentrated state.

From the above experiments with the three acids, then, it appears that they have about an equal effect in removing pain in cancerous growths; that the carbolic acid has a powerful effect in correcting the offensive fetor of cancerous discharges; and that they all have a solvent effect on cancerous tissue—the citric acid least, the acetic next in degree, and the carbolic most powerful.—*British Medical Journal, April* 21, 1866, *p.* 409.

107.—ON THE USES OF THE MEDICINAL SULPHITES AND HYPOSULPHITES IN ZYMOTIC DISEASES.

The extent and value of Dr. Giovanni Polli's researches, experimental and clinical, as to the uses of the medicinal sulphites and hyposulphites in zymotic diseases have not been duly appreciated in this country. With the cholera impending, and typhus still prevalent, and the cattle plague not yet "stamped out," we shall do good service by calling special attention to the results of Dr. Polli's labours as stated in his latest papers on the diseases caused by a morbific ferment and and their treatment, communicated to the Royal Institute of Lombardy, at Milan. His first paper appeared in the eighth volume of the *Memoirs* of the Institute, and described researches into the chemistry of morbific ferments and their treatment by anti-zymotic chemicals. Whatever be the theory as to the cause of the so-called zymosis (and either Liebig's albumenoid, or Pasteur's animalcular, or any other theory may be adopted), the practical facts, according to Polli, are, that there are two morbid elements in the blood in these zymotic diseases; the one is of the nature of a ferment or excitor of change, the other is the material capable of fermentation. When either of these is absent from the blood zymosis is impossible; but when both are present, then such a reaction takes place that zymotic phenomena result. Now, the ferment may be rendered innocuous, although it may not be readily destructible itself, by destroying the fermentable material, or by at least so changing its composition that fermentation is effectually hindered. Anti-zymotic agents are already numerous, the majority of which seem to act both upon the ferment and the fermentable material, and would therefore seem *á priori* to offer all that is needed.

But the very qualities upon which their anti-zymotic powers depend, render them dangerous in practice, because they are for the most part efficacious in proportion as they are destructive to the vital processes themselves. A safe anti-zymotic was, therefore, still needed. After a series of experimental researches Dr. Polli affirms that he has found in the sulphites and hyposulphites of potass, soda, and magnesia, medicinal agents which do not act upon the ferment, nor upon the vital processes, but upon the fermentable something without which the ferment is innocuous; and in this way they constitute safe and efficacious anti-zymotics. In the memoir under review he indicates the diseases in which they may be administered either for prevention or cure—their doses, modes of administration, and in short entire natural history, as therapeutical agents of their class.

The fundamental fact of his researches is this:—He selected a number of dogs; a proportion of these he dosed with the sulphites, while the remainder were left without. He then killed them all, the weather being warm. He found that the urine, the blood, the viscera, and the flesh of the sulphited dead dogs resisted putrefaction for many days, while the same elements of the unsulphited animals were already putrid. The obvious conclusion from so striking a fact is, that as the sulphites prevented putrefactive fermentation in the dead body, they would *á fortiori* prevent it in the living. He accordingly instituted a series of comparative experiments on the living. He selected two or three dogs as nearly equal in size and state of health as possible, and inoculated them with the nasal mucus of a glandered horse. One of the dogs had had previously administered to it from four to five scruples or more of the sulphites of soda or of magnesia daily for some days; another was treated by sulphites as soon as inoculated; a third was left without treatment, either prophylactic or curative. The result was that the third animal suffered much more severely than the other two. The like results followed after injecting pathological ferments into the veins, such as fresh and putrid pus, and putrid blood. Encouraged by these results, Dr. Polli proceeded to test the effects of the sulphites and hyposulphites on men in health and disease. He found in man, as in dogs, that they are not ordinarily decomposed in the stomach, but pass unchanged into the blood, or at least appear in a brief period unchanged in the urine, and only after a while as sulphates; that they may be traced in the organism for from 24 to 36 hours after being taken; that they slightly increase the renal secretion, and are mildly cathartic in large doses.

The clinical researches extend over a series of local and general forms of zymosis, and have been conducted chiefly by Italian physicians. That the salts have been extensively used is deducible from the fact that three chemical houses in Milan have sent out more than 1000 kilogrammes (2204 ℔s. avoirdupois) of the sulphite of magnesia—a salt which Dr. Polli says can have no other use in the arts and pharmacy in Italy than those he has indicated. We will note the uses in the order Dr. Polli arranges them.

External Uses.—In all suppurations, sloughing, and ulcerations the solutions are useful in soothing pain, removing fœtid odours, and exciting healthy action. The sulphites of soda and potass are the best; they neither stain nor corrode the linen, as the solutions of metallic oxides, and being without smell are not offensive, like the chlorides, phenic acid, &c. They have the advantage, also, of being highly soluble. In gangrene and sloughing, foul ulcers, and suppurations of all kinds, a more or less concentrated solution of the sulphite of soda, is the best. This salt is soluble in four times its weight of water, but it may be advantageously used in the proportion of one part salt to five to ten of water. The solution may be applied as a wash, or lint or a compress wet with it, or it may be injected into sinuses and sloughing cavities. The solution may also be mixed with glycerine, and the powdered salt may be sprinkled on the part affected. The sulphites of lime and magnesia have a somewhat caustic action, and are, therefore, best adapted for indolent ulcers. The immediate effect of the salts is to arrest putrid fermentation. In cases of purulent absorption (pyæmia) and septicæmia the internal use should be combined with the external. Dr. Polli recommends the sulphite of magnesia in thirty-grain doses every two hours.

2. *Pyœmia* and *Septicœmia*, and all the class of fevers, with putridity and purulent absorption, such as Hospital fever, puerperal fever, anatomical inoculation, and the like, have been treated by the sulphites with advantage. According to the clinical observations of Professor Burggraeve, of Ghent, it is necessary in those cases to give the sulphite of magnesia, so as to saturate the organism rapidly with it, and cause the fluids of the body—urine, saliva, sweat—to contain it. Half-drachm doses every one or two hours may be given with this purpose. Reports from twelve Italian Physicians testify to its beneficial effects in ameliorating all the symptoms of 18 cases

of purulent infection. Dr. Polli recommends it to be given as a prophylactic in Surgical cases, bad cases of labour, &c. From four to six drachms of the sulphite of magnesia or the hyposulphite of soda may be given during the first 24 hours in divided doses, and from six to nine drachms during the next 24 hours to prepare a patient for an operation or a labour. If, when the patient is attacked, there is excessive diarrhœa and vomiting, these must be relieved, otherwise the system will not be saturated, or else clysters of the solution must be administered. A little caustic magnesia added to the solution of the sulphite of magnesia causes it to remain better on the stomach. For the the diarrhœa opium may be combined. After a while the remedy being retained will take effect in relieving the diarrhœa, and the opium may be discontinued.

3. *Endemic* or *Miasmatic Fevers* are numerously reported on by Dr. Polli's correspondents; in some instances in a tabular form, as to hundreds of cases of quotidians, tertains, and quartans treated by the sulphites. Comparative trials of quinine and sulphites were made by some physicians both for the prevention and cure of intermittents. The results of those observations are very striking. The sulphites were found to cure a larger number of cases than quinine; to cure more completely and with less tendency to a return of the fevers, and to cure radically cases that were utterly rebellious to quinine. In others of this class the conjoined administration effected a cure when neither cured singly. The sulphites do not, like quinine, cut short the fevers; they gradually diminish the intensity of the symptoms. The sulphite of magnesia is the most efficacious, and may be taken to the extent of an ounce or an ounce and a-half in the twenty-four hours in divided doses, either in a cup of water, or in sugar, or any confection. It may be taken during the accession as well as during the remission. A change for the better, if the remedy is likely to be efficacious, is manifested in from three to five days. All acids and acidulous fluids are incompatible with the sulphites, and should therefore be forbidden. To prevent malaria taking effect, Dr. Polli recommends a solution of the hyposulphite of soda to be taken in any vehicle which serves to mask the bitter taste of the salt. A prophylactic mixture (the "*liquore solfitico antifebrile*") of about a litre or 1¾ imperial pints, containing fifty doses, may be thus made:—Hyposulphite of soda, 7721 grains; water, 9765 grains; tincture of essence of anise, 2316 grains. Two small tablespoonfuls to be taken every morning fasting in a cup of water. It may act on the bowels for the first day or two; but this effect ceases in a while, and the remedy is borne perfectly well, although a good deal of sulphuric acid is formed in the intestine. When the remedy is not effectual in a few days, in a case

of intermittent, quinine or iron may be combined. Its prophylactic value is increased in anæmic persons in the same way.

4. *The Exanthemata and Typhus Fevers.*—Dr. Polli suggests the use of the sulphites in yellow fever, and prints thirty-seven reports from numerous correspondents of their value in measles, scarlatina, small-pox, erysipelas, and typhus. The general results reported are to the effect that they mitigate the intensity of the febrile exanthem, render the confluent and malignant forms benignant, shorten the course of the milder forms, and in all cases accelerate convalescence. In typhus gravior with fetid dejections and putrid symptoms, they are found useful. Dr. Polli does not venture to assert that they will prove available in the cure of cholera and plague; but he thinks there is reasonable ground for concluding that they will be highly useful as prophylactics in destroying the fermentable material upon which the malignancy and fatality of those pestilences chiefly depend, and thus rendering an inevitable attack mild. They may have no effect on the ferment, but they will deoxidise the fermentable stuff. In all these zymotic diseases the same principle applies.

5. *Syphilitic Infection* and constitutional syphilis have been efficiently treated by the sulphites, but facts are not given. Pellagra, too, and tuberculosis, it is thought, may be benefited.

6. *Cattle Plague and Epizootics.*—Dr. A. Colaprete, di Campo di Giove (the Abruzzi), has used it in the cattle-plague, and cured a third of the animals attacked. And Dr. Polli refers to other instances in which the sulphites have been administered in epizootic aphtha of cattle.

7. Finally, Dr. Polli speaks favourably of the uses of the sulphites to anatomists and others. Corpses or viscera can be preserved for many weeks in a solution of sulphite of magnesia or of soda, without change either in colour or density, or otherwise, and without acting upon the scalpel or other anatomical instruments when dissection is made; in these respects being superior to alcohol, arsenic, corrosive sublimate, or the sulphates and chlorides, of iron, zinc, &c.

Some hints as to the administration of the sulphites may be useful to those who may be induced to try them in the various diseases in which Dr. Polli recommends them. They appear in the urine twenty minutes after being taken into the stomach unchanged, but are gradually changed in the system into sulphates. The saliva and sputa contain them. They are not decomposed in the stomach under ordinary circumstances, but when they are, there is a manifest production of sulphurous acid gas. When this is the case a little magnesia must be added to the sulphite to neutralise the acids of the stomach. The sulphites of soda and magnesia are tolerated in large doses if dissolved freely in water. A concentrated solution is apt to lie heavy on the stomach and cause a frontal headache; the more diluted, as a general rule, the better. They are decomposed by all the vegetable acids, however weak. Hence their beneficial influence will be counteracted if the patient takes citric, tartaric, malic, or oxalic acids, or foods or drinks containing them. This is important to remember when administering them in fever, inasmuch as lemonade, imperial, apple tea, and the like, are all incompatible drinks and destroy their efficacy.

The hyposulphite of soda is much more purgative than the sulphites of magnesia and soda, which are commonly more diuretic than cathartic in their action. There are individuals, however, whose intestinal canal is very sensitive to their action, in which cases a carminative and sedative are required to be combined. The purgation is, however, most commonly without pain or discomfort. The stools of those taking the sulphites lose all cadaveric or nauseous fetor. Upon the whole, the best mode of administering the sulphite of magnesia is in the form of powder, because of the large quantity of water it requires for solution, and because in that state it is most easily altered by the air. This sulphite has the least taste, and is the most active of all, and since it undergoes in the dry state the least change, it is to be preferred to all the others for medicinal purposes. From fifteen to thirty grains may be be prescribed in powder, to be taken either in water or other vehicle, or it may be covered with gum or dextrine, and sugared and taken as troches or jujubes. The sulphites are better than the hyposulphites when a rapid curative action is required; but the latter may be advantageously substituted for the former when prophylaxis is aimed at. The great aim in administering them curatively is to saturate the system with them; and for this purpose four to five drachms daily must be the minimum quantity for an adult. Three or four times that quantity of sulphite of magnesia has, however, been administered with advantage; and a third or a fourth more (five to seven drachms) of the sulphite of soda will be borne well. Their long-continued use is apt to induce œdema and diseases of debility by their deoxidising qualities; otherwise, they have no influence on the system.

The numerous researches made by Dr. Polli and his correspondents merit careful consideration and trial by the Profession in England. Our experience, so far as we have tried the salts, is in favour of Dr. Polli's statements. It must be carefully remembered, however, that the sulphites cannot be fairly tested without strict attention to the conditions under which they can be reasonably expected to be available, and with such precautions as are required by a consideration of their chemical qualities and medicinal properties.—*Med. Times and Gazette, May 5, 1866, p.* 475.

108.—DR. HASSALL'S FLOUR OF MEAT.

[This material consists, in the concentrated form, and with the exception of the water, of the whole of the constituents of the meat previously freed from bone and visible fat; and is prepared at such a low temperature that these constituents have undergone no change whatever.]

Furthermore, in consequence of the nearly impalpable character of the powder, no mastication is required, and no greater effort in swallowing than is necessary in the case of any liquid. It is impossible but that such a material should possess high dietetic value, and indeed the view originally entertained by me of its importance has by further experience and experiment become greatly confirmed.

It is, however, not only necessary that an article of diet should possess intrinsic merits, but it must also be pleasing to the eye and palate. I am glad to state that these requisites are entirely fulfilled, and that with the condition in which the several preparations are *now* manufactured I have to express my complete satisfaction; while I unhesitatingly affirm that no article of preserved food hitherto brought out contains an equal amount of nutriment, and embraces such a combination of favourable qualities, as does my flour of meat. The samples first issued had a certain dryness or roughness, and there was a deficiency of the gelatinous element of the meat, which, moreover, had a disposition to sink in the liquid as a sediment. These objections, entirely due to *mechanical causes*, have now been completely obviated.

But the principal object of my present communication is to point out certain uses of the meat-flour, in addition to those referred to in my previous letter:—

1. A spoonful of it added to ordinary beef-tea or soup will greatly enhance its nutritive properties.
2. When taken as a sandwich, or sprinkled over or mixed up with vegetables, it is often retained by the stomach when solid meat could not be taken, or, if taken, be rejected.
3. In all cases in which food has to be administered by means of the stomach-pump, as in many cases of insanity, when made into beef-tea, soup, or cocoa, it will be found a most valuable nutrient.
4. A similar remark applies where nourishment has to be conveyed into the system by enemata.
5. In all cases in which mastication is impeded from any cause—as from disease of any kind affecting the action of the jaws, or from defective teeth—it affords a ready means, as yet unequalled, of giving nourishment.

6. It is most valuable as a nutrient after parturition.
7. It is found most serviceable in many cases of dyspepsia.
8. There is no form of nourishment short of the solid meat itself so suitable in diabetes. It is scarcely necessary, however, to remark that the meat-flour should be used pure, and that neither starch nor sugar should be added when preparing from it beef-tea or soup.
9. It promotes nutrition, and so hastens recovery, in low fevers.—*Lancet, April* 28, 1866, *p.* 469.

109.—ON THE CONCENTRATION AND PRESERVATION OF MEAT.

By Dr. Arthur Hill Hassall.

Of every four pounds of the flesh of fresh beef or other meat, freed from bone and visible fat, nearly three-fourths, or three pounds, consist of water, the remaining pound containing the whole of the constituents of the meat—viz., the albumen, fibrin, gelatine, interstitial fat, creatine, creatinine, sarcin, the various phosphates, and other salts of the blood, both organic and inorganic, &c.

It long since occurred to me that, if I could succeed in removing the whole of the water of the meat without essentially altering its composition, I should obtain a material but little prone to change or alteration by keeping, and possessing a high dietetic and medicinal value. For some years, but more especially during the past few months, I have been engaged in conducting a series of experiments with this object, and, I am happy to be enabled to state, with complete success.

Into the details of the process it is unnecessary here to enter at length. I may state, however, that the meat is dried without the loss of a particle of any one of its constituents, for the most part at a temperature below the coagulating point of albumen; and that the product so obtained is then ground and dressed into a very fine powder, constituting, in fact, "*a Flour of Meat.*" This powder, when prepared from beef, is of a light-brown colour, of a very agreeable taste, and one pound of it represents about four pounds of the flesh of meat, or six pounds of the leanest joints not deprived of bone and fat; and the purposes for which it is more especially adapted are—

1st. For the speedy preparation of beef-tea.
2nd. With the requisite vegetables and flavouring substances, for the speedy preparation of soups.
3rd. Combined with farinaceous matter, it forms a highly

nutritious food, well adapted for children, the dyspeptic, and invalids.

4th. Mixed with cocoa, it furnishes a highly nutritious breakfast beverage.

5th. With other suitable ingredients, it forms an admirable meat biscuit.

Recent investigations have fully established the fact that clear beef-tea and soups contain exceedingly little solid matter, and that they are, in fact, destitute of all the nitrogenous and protein principles of the flesh, excepting gelatine, which is present chiefly after prolonged boiling, and, consequently, they are capable of affording in themselves notwithstanding their agreeable flavour, scarcely any nourishment.

The beef-tea and soups prepared with my concentrated meat, beef, veal, a mixture of these, or any other suitable kinds of meat may be partaken of in two forms, either in the clear state, when they will resemble somewhat closely the clear beef-tea and soups as now prepared, but they will differ from them in their greater strength, and in the rapidity with which they may be prepared; or, as I would more particularly recommend, they may be consumed while still containing the powder of the meat suspended in them.

The saving of time in the preparation of beef-tea is obviously a matter of considerable importance when invalids are concerned, and it is of no less consequence to the cook in the case of soups, the length of time required in their preparation being a very great obstacle in most families to their frequent consumption. The various vegetables and seasonings required for soup are likewise prepared in the same manner as the beef or other meat, and these are added to the meat in certain proportions, so that the cook has nothing to do but to allow the mixture to simmer for a few minutes, when the soup is ready for table, the solid materials being strained away if it should be desired that it should be clear, but allowed to remain if greater nourishment be required.

It is almost impossible to over-estimate the importance of the concentrated meat to invalids. By it they are enabled to receive into the stomach the whole of the constituents of the meat, no mastication being required, and no greater effort at deglutition than is needed for swallowing a liquid; and the material is, moreover, presented to the stomach in the form most easy of digestion. It is not too much to say that it will doubtless be the means of saving many lives; and I am happy now to be able to announce that the several preparations and combinations to which I have so briefly referred in this communication are now being largely manufactured, and that they will shortly be submitted to the profession and the public for their approval and judgment.—*Lancet, Feb.* 17, 1866, *p.* 185.

110.—ON THE PRESERVATION OF MILK.

By Dr. J. Scoffern, Kensington Park.

[As by far the greater portion of milk used in cities and large towns is obtained from the country, any means of preventing the putrescent changes to which it is liable during hot weather are especially valuable.]

Premising that the agent used is sulphite of soda, let me state that some years ago I was much interested in the employment of sulphurous acid and the sulphites in processes of sugar manufacture. In the course of my investigations I desired to obtain a specimen of cane juice from the West Indies unchanged, and easily accomplished the desideratum by causing a specimen of juice to be impregnated with sulphurous acid. It bore the voyage from Barbadoes to Cork unchanged, as best evidenced by the fact that the normal weight of sugar was extracted from it. So strong an evidence in favour of the anti-fermentive qualities of sulphurous acid was impressive. Therefore when lately consulted by a London purveyor of milk relative to the troubles he experienced from fermentive and putrescent causes, the experiment with sugar-cane juice naturally came to mind. For many reasons, some of which will be obvious to your readers, uncombined sulphurous acid would in the case of milk have been ineligible. Sulphite of lime was first used, but with indifferent success. Sulphite of soda has answered perfectly. Necessarily winter is not so favourable a time as summer for establishing a crucial test. In the opinion, however, of the milk purveyor, who has tried the experiment, the results are conclusive. Judging from previous and collateral experience, I am induced to believe that one teaspoonful of saturated aqueous solution of sulphite of soda added to every gallon of milk will preserve the latter from change, for many weeks at least, in the hottest days of summer.

My medical brethren need not be reminded that sulphite of soda in the proportions noted is quite harmless, and indeed imperceptible. It is not new to therapeutics as a valuable agent in pyrosis and certain forms of dyspepsia, though in these cases I find the sulphite of lime to present advantages. Collaterally I may state my belief that the sulphites of soda and of lime are amongst the best, if not the very best, of all disinfectants. I would suggest their use in the cattle disease, not merely as external disinfectants, but therapeutically administered to affected animals.—*Lancet, April* 28, 1866, *p.* 475.

111.—A CASE OF RESUSCITATION, AFTER DROWNING, BY A COMBINATION OF THE PLANS OF DR. SILVESTER AND DR. MARSHALL HALL.

An interesting example of resuscitation by perseverance in artificial respiration is furnished by the following case, for notes of which we are indebted to Mr. W. H. Ellis, house-surgeon. It contains, besides, a useful hint upon the importance of careful observation of a patient for some time afterwards, owing to the tendency which exists for the circulation to be at first imperfectly performed, and probably accumulation of black blood upon the brain thereby occasioned.

E. C., aged twenty-five, artificial flower maker, was brought to the Great Northern Hospital about half-past twelve on the morning of Jan. 28th, 1866, having been taken out of the Regent's Canal in the Caledonian-road. When first called, Mr. Ellis was told that the woman was dead, and to all appearances she was so, no pulse or respiration being perceptible. However, with the assistance of some gentlemen from St. Bartholomew's Hospital, who were in the building at the time, the wet clothes were quickly removed, and artificial respiration commenced after the plans recommended by Drs. Silvester and Marshall Hall: that is to say, at the time the arms were brought down in contact with the chest according to Silvester's method, the body was half rolled over according to Marshall Hall's plan; at the same time measures were taken to keep the patient's mouth open and her tongue well drawn forwards, and endeavours made to excite respiratory action by the application of ammonia, &c. to the nostrils. After persevering in the above methods for between ten and eleven minutes, the operators were rewarded by symptoms of returning animation, the patient making a very feeble attempt at an inspiration. The process being continued for a few minutes longer, she was sufficiently restored for brandy, ammonia, &c., to be administered. After this she was removed to bed and wrapped in blankets. Hot-water bottles were then applied to the thighs, axillæ, and feet, with mustard poultices to the abdomen and calves of both legs. Friction with hot flannels was now resorted to in order to keep up the very feeble and intermittent pulse, and this being continued for an hour, she was left in charge of a nurse, with instructions to continue the administration of beef-tea, stimulants, &c. At about two a.m. the nurse reported that the woman was dead. This proved to be a false alarm; but she was found in a state of the most perfect collapse, both respiration and circulation apparently being at a standstill. Artificial respiration was again resorted to, and after a few minutes she gradually revived. From this time till four A.M. she was kept sitting up in bed, constant friction being applied to the trunk and extremities; for no sooner did she lie back in bed than her pulse was imperceptible, and the respiration seemingly gone. After four o'clock she improved rapidly, the respiration and circulation evidently becoming stronger and more regular; and about half-past five she was allowed to sleep, as she was getting warm and breathing well. About nine A.M. she seemed quite well. She left the hospital two days afterwards, not having had a bad symptom.

The chief difficulty experienced in applying Dr. Silvester's method was in overcoming the extreme rigidity of the muscles. —*Lancet, May* 5, 1866, *p.* 483.

112.—ON THE ACTION OF FUNGI IN THE PRODUCTION OF DISEASE.

By Dr. TILBURY FOX.

[There are many questions connected with the action of fungi in the production of disease which are most interesting as well as important. Thus it is questionable whether these so-called parasites are vegetable or not in nature, and whether they are distinct species, or only varieties of one species. It is now admitted by all that the fungi are really of a vegetable nature, and that their germs are derived from the exterior.]

The mode of entry of the Fungus into the System.—There is no difficulty in accounting for the access of germs to living bodies, for these germs are freely distributed and disseminated in the air. The best illustration of this fact may be noted in the experiments of M. Bazin (Gazette Méd. de Paris, July 30, 1864), which consisted in passing currents of air over the head of a favus patient, and thence over the open mouth of a jar containing ice. The ice cooled the air, causing the deposition of moisture, in the drops of which the achorion sporules were detected. The same thing may be shown by holding a moistened glass slip near the head of a patient, and just rubbing his scalp freely. Of course, actual contact is much more effectual in the implantation of germs. But, without delay, let us suppose that the sporular elements find their way to the human surface; how get they deeply into the tissues? In various ways probably. Let us take a general sketch. The greater the degree of moisture and heat, the better is the chance of entrée.

First of all, the fungus elements may enter by fissures or natural orifices; for example, in ordinary ringworm the sporules lodge themselves at the opening of the hair follicles, and presently get beneath the epithelial scales. We shall see, directly, how. A great many experiments have been made at different times, upon this point, in the case of plants. De Bary (Die gegenwartig herrschende Kartoffelkrankheit, ihre ursache und ihre Verhutung. Leipsic, 1861), found that, in terminal filaments of potato mould, so-called zoospores were formed, which bud, protrude filaments forming a mycelium which has the power "of penetrating the cellular tissue in twelve hours, and when established there it bursts through the stomata of the leaves." This "boring" operation is quite likely to occur, especially where the structures are diseased: as, for example, the muscardine in silkworm, in diseased mucous surfaces or epithelial changes; here the entrance by continuity is easily accomplished by the growing filament. It has been supposed that mycelium may get within the shaft of the hair in some part of its course, in this way. I do not believe it. If a fungus finds an entrance, it is either through a cut end, a distinct fracture, or, what is usually the case, the soft growing root. A good deal of doubt has been expressed as to whether the spores could find their way into the interior of plants through the stomata. It seems pretty clear that the latter are not sufficiently large for the occurrence. There can be no question, however, that, in a large number of instances, the spores send out little processes, which get into the plant through the stomata. Here the recent experiments of De Bary help us again. They are noticed by Mr. Cooke in his admirable popular work on microscopic fungi. This observer took a large number of common garden cress plants, placed their roots in water containing zoospores, and though the former became covered with these latter bodies, yet not a jot of evidence of penetration occurred. De Bary, however, found that if the cotyledons or seed-leaves are watered with fluid containing zoospores, that slender tubes put out by the zoospores, enter the stomata, the terminal ends enlarge, branch out, and become the centres from which a ramifying mycelium is produced, which presently shows itself externally. De Bary tested 105 plants in like manner and under similar circumstances, with water free from zoospores, and without the production of any sign of rust in these. De Bary concludes that plants are not infected by spores entering through the roots or leaves, but through the medium of the seed leaves of cotyledons. But it is probable that the fluid contents of the spore cells may be absorbed and give rise to disease. The Rev. Mr. Berkeley, several years ago, found that the germs of bunt placed in contact with seeds, infected them, without there being any evidence to show that any spore or mycelial thread had effected an entrance. It seemed as if the granular fluid contents were taken up by the plant and caused mischief. It is possible that minute threads might have penetrated the seeds nevertheless. There is, however, no difficulty in supposing the granular contents of spores (sporules) capable of reproducing the typical spore. But, in the next place, there cannot be a doubt but that in the human subject the germs of the fungus find their way to the roots of the hairs, and are carried bodily upwards into the shaft in the process of growth, developing as they go, till at last they degenerate and break up the fibrous structure in which they are. By analogy we should quite expect that such a thing is possible, and, indeed, of frequent occurrence in the case of the tender roots of plants ; and this is more likely to happen when the contents of the original spore (which is as large as the spongiole cell) happen to be discharged by bursting. Moreover, it is quite clear that the germs of parasites enter at a much earlier period than we are apt to imagine, and lie dormant, brooding mischief till the favourable opportunity arrives. De Bary proved this in the case of the white rust (cystopus) which hybernates as it were in the sub-epidermal structures during the winter, till the spring arrives. In addition, the fungi "make head," so to speak, into structures in virtue of the chemical action which they set up. This is best seen in the hard structures of animals. Carbonic acid is given off at the terminal cells. This dissolves the lime of the shell and allows the parasite to effect an entrance most easily. The experiments of Wittich, quoted by Robin, all tell in the same direction. Panceri has come to the conclusion, however, that, in the case of the egg, the minute germs effect an introduction through minute microscopic holes which exist in the shell. *Lastly*, traumatic lesions afford an easy channel for the conveyance of fungi to deep structures. This is what happens in the mycetoma or fungus foot of India.

We have then, as modes of entrance—(1.) That through natural orifices ; (2.) That in which the growing force forces the mycelial thread beneath the layers of the superficial tissues ; (3.) That in which processes shoot out from the spore and enter by such openings as stomata ; (4.) That where the cell contents are absorbed ; (5.) That in which the spores are carried bodily inwards by growing parts ; or (6.) dissolve away the opposing structures by chemical action ; or (7.) enter by traumatic lesions. In each and every instance the germs of parasites are derived *ab externo* and not generated *spontaneously.*

There are some special circumstances that deserve comment. It has been asserted that microscopic entophytes have been discovered in close cavities utterly cut off from communication with the external air. But these instances are open to grave objection ; fungi have been found in the fluid of the ventricles of the brain, which, however, was allowed to stand all night exposed before it was examined. Again, it is asserted that in the kidneys the like has been found. This is open to exactly the same objection. The case of the egg parasite has been explained away by Panceri; and it has yet to be shown, supposing the urine has ever during life contained fungus elements, that air cannot enter the bladder. The case of germs of vegetable nature in the blood current presents some difficulty; but even here the most considerable caution is needed. We know that fungi spring up with enormous rapidity; and it must be proved that those spores and mycelia are present at the moment of death, nay during life, before we can give credence to any theory which asserts that they have been present and introduced during life, and not by a communication with the external air. It is still a question whether the endosmotic action of the villi may not be able to account for the presence of cryptogams in the blood current. As far as the facts of vegetable parasitism go, we are bound to deny any such occurrence. And, upon analogical grounds, I venture to assert that the entozoa found in muscle, which have lately caused no little sensation, are not vegetable in nature. Should they be proved so, it will entirely alter the whole subject of vegetable parasitism; for we are justified at present in asserting that there is probably no known instance of a growing plant in any situation not in direct or possible communication with the air. I am bound to say that Dr. Thudicum believes in the vegetable nature (see Report of Medical Officer of Privy Council for 1865) of the rinderpest entozoa (?).

I pass to the consideration of the part played by fungi in diseased states. Two theories the most opposite in intention have been held by writers and others, so opposed that really the conclusion is forced upon one that both *must* be wrong and a middle belief correct. Whilst one batch of inquirers affirms that parasites are accidental, another contends that they are the essential cause of those diseased conditions found in "association" with their growth. Ehrenberg, in speaking of organized parasites at a time when the exact nature of many of them was indistinctly recognised, said, "that there is more cause for wonder at the limitation of their effects by the actions of living bodies they inhabit, than at any morbid effects they appear actually to produce." It must first of all be noted that there are certain conditions which are peculiarly favourable to the growth of vegetable parasites. The latter are ubiquitous, capable of resisting the action of heat, cold, and decomposition, have a tremendous and rapid power of increase, and will remain for a very long time in a state of inactivity; yet, notwithstanding all this facility, there are certain states of organisms against which they fail; which will somehow resist their inroad and attacks; and it is now clear that though parasites may for the moment get a temporary hold, yet they will not flourish upon a typically healthy surface. This is a fundamental truism that must be observed in reference to therapeutics. For rusts and mildews prevail in direct ratio to the wetness of the season, or after drought, as in the pea or hop; damp itself is very favourable, and where there is much drought the vigour and the circulation of plants are diminished very considerably. When plants are very ripe also, there is a less degree of vitality present, in consequence of the cessation in great extent of the circulation and vital connexion between the fruit and the stem. The same thing holds good in every instance where animals, plants, or men are attacked. We may instance the case of muscardine. The experiments of Claude Bernard also showed that frogs kept in captivity got out of order, and aphthous conditions arose. A healthy frog brought near its diseased fellows "set contagion at defiance," but unhealthy frogs were at once attacked by the vegetation flourishing on the aphthous surfaces of others; and the case of favus in man, or scab in sheep, of which an account may be seen in the Gardener's Chronicle for April 24, 1864, is illustrative of the fact under notice.

There is always a certain resistent power about all healthy living beings; and a certain amount of fungus, however it acts, may be present without giving rise to what one can possibly call disease. In young life, of course, one would expect that fungi would obtain a hold more effectually than in old life; and it is very remarkable that the white rust before referred to, according to De Bary's experiments, should effect an entrance into the system of the garden cress, by attacking the young leaves or cotyledons. The young and tender stage becomes an easy prey; and this is exactly what we find in the human subject, the young being most liable to ringworm.

Taking all things into consideration, it is clear that parasitic disease, or—as I have named it generically *tinea*,—cannot be explained by either of the conflicting theories I have referred to, but consists of three distinct components, which must be recognised, if the physician would cure his patient well and quickly.

1. A certain state of soil: in speaking of the polymorphism of fungi, I noticed that each fungus appeared to require each its special kind of pabulum.

2. The access of air, and the presence of heat and moisture, —the conditions necessary to support the life of fungus. And,

3. The introduction from without to and action upon the body of the vegetable germs.

The first and second will be passed over without comment: my remarks are specially intended to define the action of the parasite in the production of diseased states. Now fungi are not "accidental" and unimportant, but act in several distinct ways when once they take hold and grow upon the surface. This is important; if we insist upon some *one modus operandi*, we shall assuredly find our position utterly untenable. They act then (often in more than one way in the same instance be it remembered),—

Firstly, mechanically.—If you simply rub into the surface some of the fungus elements, in many cases you get what we know as irritation. This is seen in the ordinary herpetic ringworm of the surface, where the mycelial threads range over the skin beneath the epidermis and lead to erythema, &c. A very remarkable case is recorded by Dr. Kennedy of Dublin, in which a quantity of flax powder was inhaled by a lad who became attacked with measles and peculiarly severe local dyspnœal symptoms, evidently dependant upon the direct mechanical irritation exerted by the fungus elements. In the case of mildew of plants the same thing is seen, the threads of the mycelium grow and force asunder the tender structures near it. Now, it is this mechanical action exerted by the growing force which is at work, especially in ringworm. The fungus finds its way to the sub-epidermal space, from thence to the hair follicle, irritating and interfering mechanically with the growing parts, enters into the hair, and by its increase and development simply splits up the hair shaft, appropriating also its juices, and rendering it all the more brittle, and therefore the more easily destructible. To declare in such a case that the parasite is accidental in any sense of the word is to turn a deaf ear to the plainest voice of facts; but this very action can be isolated. I have performed a good many experiments at different times with diseased hairs out of the body, and occasionally it is possible to get a hair containing spores, which spores will germinate and actually produce the splitting up of the hair, and the other changes that are observed in ringworm. In fact, *to produce the lesion of ringworm out of the body.* In those instances in which the mycelium abounds, the epithelium seems to suffer particularly. On the mucous surfaces there are no such structures as hairs which form a lodgement, so to speak, for the fungi, and hence no marked results are visible. The cells of the tissues are invaded and destroyed, the mycelial phase abounds and ramifies in the secretion, and not in the tissues themselves; but there is the same *capability* of damaging when parasites attack only the internal surfaces. If we would wish for examples of the enormity of the force exerted by a growing fungus we have only to confine some of the more ordinary varieties and see the result. Now, it so happens that no other agent can produce in disease the same kind of action as that exerted by a growing fungus,—such as splits up the hairs in the way in which this is observed in tinea; and it is this state of things which I regard as the pathognomonic lesion of ringworm, viz., the mechanical action of the parasite upon the hair and epithelium, in connexion with other minor changes.

One word as to definition. I use the word tinea as the generic term, and particularize each variety by the terms favosa, tonsurans, sycosis, vesicolor, circinata, &c. The tinea signfying especially the diseased state of the hair and epithelium. Now, take the case of sycosis, which means inflammation of the follicles of the chin and lips. It may not be caused by a parasite; but undoubtedly cases are sometimes caused by a fungus, and these I called tinea sycosis. Again, tinea circinata means the parasitic herpes circinatus, and tinea decalvans the baldness produced by the fungus (microsporon Audouini), as distinguished from alopecia, non-parasitic baldness, the result of many different causes. The term tinea is very distinctive.

Secondly, Fungi act by inducing local chemical change.—They absorb oxygen and give out carbonic acid, and, as has been before observed they hereby secure to themselves the power of penetrating calcareous structures. In addition, a large amount of gas is evolved as in cases of sarcinal disease. Moreover, they lead to fatty degeneration. If any one will take the trouble to examine carefully some of the old stubs in favus, he will notice a certain amount of fatty changes going on in the cell structures. Remove a hair of this kind loaded with sporules, and get the latter to germinate, and the fatty alteration goes on at a rapid rate, till after a time a large quantity of crystalline fat is produced. Now, this will not happen unless the fungus germinate; but happening, it is worked out in accordance with the views lately put forth by Dr. Bence Jones, and was expressed in precise terms in my book on parasitic diseases. It has been remarked by many observers that fat is always present in considerable amount in connexion with the development of fungi. M. Signol believed that fat very much favoured the development of bacteridia. Perhaps the very best exemplification of the association of fatty change with parasitism is that afforded by the case of the madura foot, where the oily matter is so very abundant. The tissues degenerate, and the crystalline fat is so varied and peculiar as to have actually misled observers into the belief that it was a form of fungus. Now, it becomes a question whether fat assists the development of fungi, or whether the latter attract fatty matter, the fungus forming a centre of attraction for crystallization, or the fatty change be the result of cryptogamic growth. I adhere to my original belief, and Dr. Carter is of the same opinion, that the fatty change is coincident with and a consequence of the growth of fungi. Nitrogenized and other matter becomes fatty in this way very readily. Of course, under such circumstances the fungi become a centre of attraction for the fat. It is a chemical action entirely, as far as the degeneration is concerned; a process of oxidation which the fungus induces under favourable circumstances in connexion with the performance of its own vital functions.

Thirdly, Fungi act as conveyers of poison.—This is a mode of influence which has been altogether disregarded by observers. If the endogenous pus cell can convey the noxious poison of an acute disease, why may not the elements of a fungus act in a similar capacity? Recent research has shown that all fungi exhibit great transportability. Now, what action have the cells afloat in the air of hospitals during the time of epidemics, such, for instance, as cholera (see Dr. Thomson's Observations at St. Thomas's in 1854); may they not take the virus of a hospital gangrene from one patient to another, acting the part of a fomes in the very same way, comparatively speaking, that man himself does? Suppose we inoculate with fungus elements, it is clear that in some instances symptoms ensue (as in Dr. Kennedy's and Salisbury's cases) before the onset of local symptoms. Again, the fungus elements would appear to be most active in their early stage, that is to say, when the poison produced simultaneously with their development is in its freshest and most active condition. Again, respirators in epidemics have been found to be efficacious. And, lastly, direct experiments, upon plants especially, have shown that disease may be produced by the contact of fungus elements, when there is not a particle of evidence to prove that sporules, spores, or mycelial threads have entered the organism of such plants, but where there is the greatest probability that the granular and fluid contents may be the poisonous compound which, when absorbed, gives rise to the subsequent malady. It is not unlikely that in catarrh and influenza especially such a conveying property may be at work. We have the strongest possible amount of analogical evidence in regard to animal life, comprehended in all the details of the "animalculæ theory of disease,"—a doctrine that may be pooh-poohed by some, but which must erelong be fairly discussed. One might give a great deal of very interesting matter under this head. Those who are interested in the subject should read Sir Henry Holland's article in his Medical Notes and Reflections, 4th edition, I think, on the Animalcule Theory of Life, and to Dr. Daubeny's essay in one of the volumes of the Edinburgh Philosophical Journal, some few years back. The occurrence of epidemics, be it noted, moreover is often associated with the peculiar prevalence of various moulds and mildews,—a source of terror and superstitious horror in bygone time, which gave rise to the idea of a raining of blood. Plutarch refer to such an occurrence in the plague of

Rome. Hecker, in his work on the Epidems of the Middle Ages, also associates it with the disasters of 789 and 959. The spots were actually observed on garments, and called lepra vestium; *signacula* was another term. In 1502 and 1503, it again frightened everybody. Agricola was certainly one of the first to give a rational explanation, he attributing it to the appearance of a lichen. The fungi attacked walls, bread, cheese, meat even, and garments, in Venetia, in 1819, and also articles of food, and garments, and all fruits, during the years following to 1829. And is there not something similar observable now-a-days? Have we not had some very remarkable and severe epidemics, and have not fungi been remarkably abundant on vegetation? I will not theorise, but merely just draw attention to the coincidence. The particular action of fungi now under notice will perhaps be better appreciated in connexion with that now to be described.

Fourthly, which looks upon these organisms as developers of poison, and comprehends Dr. Richardson's forsaken theory of zymosis,—a doctrine that appears to me most satisfactory. It has been suggested at different times by one and another observer that the fungi themselves induce change *actually in the circulating current* sufficient to account for disease, either by setting up a kind of fermentative action in the blood, giving rise to the production of a specific compound,—a poison, in fact, just in like way to that which happens in ordinary fermentation, or setting up change by catalysis,—a wonderful enigma. Others affirm that no poison is produced in the body itself, but that the fungus helps out its increase when once introduced into the system. For my own part I cannot believe that any very important change could be induced by the growth of fungi in the blood current. The presence of air is so very necessary; and not only mere presence, but such as is implied by a direct communication between the growing vegetation and the external air. Outside the body, or in the cavities which communicate with the air, many very important and frequent changes are induced without a doubt.

Dr. Salisbury is a careful observer. He declares, and as far as I know holds to his opinion, that a form of disease, if not identical, at any rate very like measles, results *under certain circumstances* from the inoculation of the fungus of wheat straw. Dr. Kennedy has given confirmatory evidence. Does the fungus *per se* produce the result, or is it a conveyer, or is it the producer of the poison outside the body in the musty straw?

Dr. Richardson, quoted by the late Dr. Barker of Bedford, records the onset of erysipelatous mischief from a like cause. In France the most serious inflammatory mischiefs of veins and lymphatics have followed wounds inflicted with instruments used to cut off the diseased vine-shoots. Dr. Collin, the medical inspector of the waters of the St. Honoré, Nievre, records even fatal results. MM. Demartes and Bouché of Vitrany have also investigated this subject, and conclude that the oidium *can* produce such mischief, but they suggest some sort of coincidence between the special development of the oidium and the occurrence in greater frequency of inflammatory disease. It is to be hoped that the French Academy will, now it has taken note of the subject, enter into a full investigation of it. It is true that ill effect does not always follow experiments with the oidium. MM. Speneux and Letellier failed to produce anything beyond a little redness and irritation by inoculating people with the rasping of leaves diseased by the odium (Pract. Jour. of Med. and Surgery, Nov. 1864); and MM. Leplat and Taillard on the one, and M. Wertheim on the other hand, came to opposite results by injecting fungus elements into veins of dogs and other animals. There can be no questioning that some fungi are more hurtful than others, and much depends upon the concomitant conditions. The arunda donax, the large red reed of the south of Europe, is attacked by a black rust, and those who cut the reeds suffer from very violent headaches; it is affirmed by M. Michel that the spores produce a papular rash on the face, with much swelling, and a good many serious general symptoms (Yearbook, 1861 2). It would seem that the fungus, *per se*, is not sufficient, but that there is something in addition which is intimately connected with the vitality of the fungus. This would seem to be taught by the case of bacteridia. Whatever they be, no injurious results happen unless the medium itself in which they exist contain some peculiar virulence of its own. Just as in the case of inflammatory attacks caused by oidial inoculation. The power of vegetable organisms to induce transformation, which must of course be accompanied by distinct chemical change, has been well exemplified by an experiment of M. Lemaire, who took some beans, placed them on a moist sponge, and found that bacteridia soon sprang up, before germination, being succeeded by monads and vibriones; and the like happened after the soil used had been heated to a red heat. Now, if a small quantity of phenic acid (which has the property of suspending infusorial development) was added, the germination came to a standstill until the phenic acid evaporated, when it recommenced. M. Pasteur's experiments on acetic fermentation tend to the like result; and M. Trècul's observations lead to the belief that the change induced in solutions by fungi, as in the case of alcoholic fermentation, depends upon the performance of the nutritive act of the vegetable cell. The fact is, the fungus, when growing, necessarily decomposes the medium, and induces chemical change, whilst the result depends upon the composition of the material acted upon. In like manner, it is conceivable that the fungus of wheat straw acts upon the juices of the stem, producing some subtile compound; bacteridia do the same in *sang de rate* and the oidium in the vine disease.

It has been supposed that the poisons of measles, influenza, cholera, nay, asthma, and some other acute diseases, may be produced in the way indicated; but it must be remembered that two or more of the modes of action already noticed may be conjoined; that is to say, a fungus may act mechanically as a conveyer and developer of poison at the same time, and in one case. But not only acute but chronic diseases are produced. I refer to the large class of instances in which vegetable parasites induce slow changes of deleterious nature in articles of diet, giving rise to "ergotism." Bad grain, bad potatoes, bad rice, bad maize, are illustrative. The late Russian epidemic, the Irish fevers, pellagra in Lombardy, gangrene in sheep and beasts, ergotism in horses, have all been regarded as taking origin especially from the play of ergotized foods. In the group of *chronic* maladies the material acted upon by the fungus is a solid. The access of air is not so perfect nor so free; the moisture is considerably less; all of which tends in great measure to account for the difference of the quality of the resultant morbid product. The productiveness of grain so infested is considerably lowered. Sir H. Davy proved this long ago. He found that diseased wheat yielded from 21 to 65 per cent. of nutritious matter against the 95 per cent of the healthy grain. It has been suggested at various times that the degeneration of rice by parasitic action gives rise to the formation of products which occasion very severe symptoms of intestinal irritation, resembling dysentery, and that œdema of the leg often follows. And it is not unlikely that the many peculiar ulcerative conditions of the lower extremities are favoured by the quality of food and induced in like manner. The diminution in the productiveness of the silkworm affected by muscardine affords a capital instance of analogical occurrence. The statistics issued by the Chamber of Commerce of Turin show that although formerly about some 650,000 myriagrammes of cocoons were produced in the country, in 1864 there were 525,000, and last year 283,000 only. I have been paying some little attention to the case of mildewed cotton, —hunting after *illustrative* facts,—and I find that the germs of mildew are really present in the cotton in its rough state, as sold in the market before it reaches the manufacturer. It of course is possible that the processes through which it passes in the hands of the latter may destroy all the vitality of the fungi, but this is not certain; but if it really does, still the fact of the presence of mildewed germs in vigour would imply the existence of a certain degree of deterioration in the actual fibre itself, perhaps induced by the bad cultivation or growing of the original plant,—a point of no mean interest to the merchant. It would make the fibre less able to resist the action of the size and other agents used in the manufacture into stuffs.

I have spoken of things going on outside the body, and then introduced to it; but within a recent time, certain facts have come to hand showing that under special conditions, though good food be taken into the stomach, yet, in the digestive tract, changes of objectionable character may be induced by the agency of fungi. I have to quote Dr. Salisbury again as my authority. He believes that chronic diarrhœa in the army is caused in this way (see the Report of the Surgeon-general of Ohio, in the Amer. Jour. of Med. Sciences, 1865). Wherever there is a poor amylaceous diet, and there be retention of the food, the torula, almost always present, grows, and in so doing induces fermentative changes, with the evolution of gas,—the production of intestinal irritation and diarrhœa,—the torula vegetating into a myceliated "algoid" mass, which may be observed in the fæces; and it appears that its amount is in direct relation to the severity of the disease; the production of sugar being rapid and detectable in the mucous tissues. The green stools of children are so produced, and Dr. Salisbury thinks also that semi-paralytic symptoms ensue. The case of sarcinal disease is on a par entirely; deranged digestion, detention of food, the presence of penicillium, and the evolution of gas with the formation of sarcinæ; vomiting is the result of gastric, as diarrhœa that of intestinal irritation. The stomach in the former, and the intestines in the latter, getting semi-paralysed, at least losing tone and getting relaxed. In both cases there is the mechanical action of the fungus and the induction of chemical changes within the body. The case of diseased foods is one of surpassing consequence, and deserves all the attention we can afford to it.

The quality and character of the poisons or products of this fermentative act are matters of no little interest. Dr. Richardson has lately deserted the zymotic, and given his adhesion to a new theory, which regards the poisons as of an alkaloid character,—basing this position upon the supposed isolation of the pyæmic poison; however, further experiment is needed to establish the truth of the new doctrine. If the poison of so-called zymotic diseases be chemically inorganic, how comes it that nothing of the kind can be obtained by chemical analysis? The diffusion and spread of disease is opposed also to such a view. There is a power of increment about these viruses which is very marvellous and peculiar. There is also a vital principle or act which is very distinct. Another feature worth notice is this, that the effect of the poison does not seem to be, as is the case with mere chemical agents, proportionate to the dose, so to speak, but to the peculiar virulency, which varies as much as the state of the nutrition of the organism acted upon. The viruses certainly, as to their characters, vary considerably, and are not, *definite* in the way that we would expect if they were of an alkaloid nature. Independently even of these kinds of influence already noticed, fungi, in the fifth place, would seem to possess inherent noxious qualities in some cases. Just as insects have the power of producing special poisons, so may fungi in a much less degree. The anamita muscaria affords a resinous principle, which chemists isolate. In other cases—for example, the mushroom—there is evidently an alkaloid, as MM. Sicard and Schoras have shown (Journal de Pharmacie, 1865); but the action of it is different from that of viruses altogether.

Now I have mentioned five different ways in which fungi may act; and these may be summed up as follow, being divided into those which are direct and indirect. Directly, they may act mechanically, or by inducing local chemical change; indirectly, by bringing about changes in substances out of the body, which are brought to influence the latter; by setting up a kind of fermentative action in part due to the oxidation consequent upon the nutritive changes in the plant, or by giving rise to products having an acute or a chronic action, and whose nature is at present a matter of doubt.

And now I am prepared to meet the hypothesis that parasitic disease has nothing essential to do with the development of parasites. Mr. Hunt takes the boldest view in the ranks of the opposition, declaring that the causes of parasitic disease are four, and four only,—uncleanliness, atmospheric impurities, deficient exercise, and contagion. I take my stand upon the mechanical action of fungi, and the induction of fatty changes, and defy any one to shake me the least from my footing. Mr. Hunt states that the above four conditions "poison the blood, producing not only their immediate effects in the form of parasitic skin diseases, but laying the foundation probably of more serious disorders, manifested in after life by the presence of lumbrici, ascarides, tape-worm, pediculi, fungi, hydatids, tubercles, and perhaps cancerous germs, in the various organisms." What does this mean? That these varied mischiefs have not each their proper cause, but arise from one and the same influence. This is surely either subversion of the logical definition of cause—unconditional sequence—which is so tenaciously upheld and received as our only true belief. I grant that the four states lower the nutrition of the system, and make it more fit for parasitic growth; this is only one item of the total. The true state of the case I take to be this. That there is a necessary nidus, which is exalted by some into the position of the supreme disease, to the negation of any and every effect produced by the fungus itself, which finds the soil congenial,—a soil associated with bad living, and bad hygiene of all kinds; the fungus growing acts in the various ways already detailed; in ordinary cutaneous affections, the effect upon the hair and epithelium (mechanical and chemical) being *pathognomonic.* Parasitic disease, then, is a composite affair, consisting of mal-nutrition, a growing parasite, and certain effects of such growth.

There is yet one category of facts that needs a word or two of comment, viz., the comparative pathology or the intertransmission of parasitic (vegetable) maladies. In addition to what I have given in my work, a good deal of information has been accumulating. It is now admitted that the transmission of the common ringworm of the surface from animals to man is very common. I am informed upon good authority that this is of very frequent occurrence in Australia, the milkers of cows especially being largely affected. Professor Gerlach (abstract in Ed. Vet. Rev., vol. ii.) has noticed it in dogs, horses, and oxen, and in man, but the sheep and pig seem to offer exception. Dr. Frazer (Dub. Quart. Journ. of Med. Science, May 1865) contributed a paper, "Remarks on a Common Herpetic Epizootic Affection, and on its alleged frequent Transmission to the Human Subject," containing cases. This gentleman quotes Mr. Brady, and Mr. Whitla, in reference to other instances. Dr. Fehr has noticed in Switzerland the transmission from cattle to man. I can confirm by my own experience the truth of these statements. I do not mention any old cases, such as mice, affecting man; but my friend Dr. Allchin informs me that he has seen the transmission of mange from a cat to a child.

Now, I might argue just in like manner in regard to the animal parasites. The two classes of cases are mutually illustrative of each other's *modus operandi.* I take the case of scabies. The acarus demands a suitable soil. It has been pretty well shown, in animals especially, that acari will not grow on all surfaces, but only on those whose hygienic condition we have reason to know from the circumstances that have been at play is not that of health. The limit of variation is by no means made out in the case of the animal parasites. The relations of acari on bodies generally is being canvassed, especially by German writers. The mode of entry has an analogy also. There is the same difference of opinion as to whether the acari are accidentals or *veræ causæ;* but there is plenty of evidence to indicate the iraction as mechanical irritants, and as the possible developers of irritant products. But these points I cannot now enter upon.

The matter of the action of fungi is a large and wide one; already we see enough to show that the studious inquirer will be amply repaid if he tread carefully the somewhat now uncertain path before him, and the promising indications of success are many.—*Edinburgh Med. Journal, April* 1866, *p.* 875.

113.—OBSTRUCTION OF THE BOWELS, SUCCESSFULLY TREATED BY THE USE OF THE STOMACH PUMP.

By Dr. Ebenezer Fleming, Stranraer, Wigtonshire.

[The patient was 59 years of age. Previous to the present illness he had three different attacks of obstruction of the bowels, in one of which, after the desired relief, gout appeared in the great toe of his left foot. In November 1865 obstruction again occurred and did not yield to any aperients tried, nor yet to injections. The pain was very severe and paroxysmal, and vomiting constant; pulse 72, of good volume. Abdomen much distended. Dr. Dickson, of Newton Stewart, and Dr. Fleming saw the case together.]

We agreed first to inject a quantity of soap and warm water, by means of the long tube attached to the stomach pump. The tube, measuring a yard long, passed freely and without pain its whole length, but from the distended state of abdomen, the injected fluid caused considerable uneasiness. It was retained. A small quantity of frothy mucus was passed. No other result. The following pills were prescribed, to be used during the night:—℞. Sub. mur. hyd. gr. xij., ext. hyosy. gr. xviij. M. et div. in pil. xii. One every three hours. Abdomen to be well fomented and rubbed with ung. hydr. mit.

26th, 12 noon. Dr. D. White, of Whithorn, who had attended Mr. S. in some of the former attacks, was now associated with us in the case. No vomiting since last night; abdomen more distended; other symptoms unchanged. We agreed again to pass the long tube, and throw into the colon a pint of infusion of salts and senna, with ℥ss. ol. terbinthinæ. Retained, but no result. 4. p.m. Distension of abdomen much increased, so as to impede respiration; pulse continued good. Hab. pulv. jalapæ ver., ℈ij. 6 p.m. Repeated last injection, with addition of tinct. assafœtidæ, ʒij. The ordinary enema apparatus was on this occasion used. After an ineffectual attempt at the night chair, the pulse suddenly increased to 132, and became feeble; respirations 48; abdomen distended to its utmost limit; outline of colon distinctly perceptible. The symptoms now became more and more alarming. We thought of putting patient into a warm bath, and dashing a pitcher of cold water over the abdomen, but he became so depressed that we could not attempt it. Dr. Dickson suggested that the long tube should be passed, with the hope of drawing away flatus. At our subsequent deliberations this gave the cue to our agreeing to pass the tube into the colon, and applying the stomach-pump, to try whether it would relieve the distension. 10 p.m. The long tube being attached to the pump, as for emptying the stomach, I again passed it its whole length without difficulty into the colon. The first result of the syringe was to bring away flatus, then semi-fluid fæces until a wash-basin was filled. Dr. White supported the tense abdomen, and he felt it distinctly subsiding under each action of the syringe. Complete relief was immediately experienced. Habent ol. ricini ℥j. During the night the bowels were two or three times very copiously relieved, and all the severe symptoms subsided. Next day gout appeared on great the of left foot (as on a previous attack), for which he was treated by Dr. Dickson.

A letter, dated 6th inst., from Mr. S., now residing in a distant part of the country, informs me that he is keeping well, but that he requires to take great care, and adds, "My left foot still looks gouty, and swells slightly in the evening."

Remarks.—The progress of the case corroborated our opinion that the cause of the attack was gout seizing upon some part of the bowel, causing primary contraction, but more extensive dilatation. Dr. Wood, of America, in his first volume of Practice of Medicine, refers to such cases of obstruction of the bowels, and considers them less dangerous than other forms. In this particular case, however, the symptoms became so alarming, and the ordinary treatment so inefficient, that we all felt that at any moment fatal inflammatory action might set in. In our experience or course of reading, none of us had ever seen or even heard of the stomach pump being used in this way; but it is very probable that other Practitioners have applied it similarly, although I have not been able to find a published case. Before reporting this case, I intended to have more experience of this application of the operation. Such cases, however, do not often occur in a country practice; and should similar cases be treated on this principle by other Practitioners after the failure of the ordinary routine of treatment, I should be much obliged by receiving brief reports of the result. In mechanical occlusion of the bowel, whether caused by intussusception, twist, internal hernia, tumour, gallstone, or structural change, this operation would most probably fail; but in many cases of obstruction we cannot determine the real cause during life; and other means having failed, the stomach pump might be applied as herein recommended—viz., to inject a warm fluid as a solvent or diluent, allow it to remain two or three hours, reverse action of pump, and eject—with the hope that in the worst forms of obstruction it is possible, and in the simple forms probable, that it will be successful.—*Med. Times and Gazette, April* 21, 1866, *p.* 414.

114.—ON THE SPHYGMOGRAPH.

By the Editor of the Lancet.

There are few things more interesting in the recent history of medicine than the remarkable ingenuity with which physical means of research have been brought to bear upon the hidden secrets of the body in health and disease. What the opthalmoscope has done for the eye, and the laryngoscope for the larynx, the stethoscope and percusser have done for the hidden organs of the chest. The thermometer, in the hands of Wunderlich, Aitken, and Ringer, is beginning to tell us its own story in the diagnosis and prognosis of disease; so that, for example, in the treatment of fever the thermometer will at the fourth day establish an almost absolute diagnosis between typhus and typhoid fever. And a further instrument of precision, of remarkable beauty and wide range of usefulness, was on Wednesday evening last, brought prominently under the notice of English physicians by Dr. Anstie, at the Medical Society of London. The sphygmograph of M. Marey is an exquisitely designed instrument, by the aid of which the pulse is armed with a pen, and at every beat writes its own diagram, and registers its own characters. In this diagram each part of every revolution, or "beat" of the heart is recorded, so that the relation of the systole and diastole is inscribed in every curve, and the state of arterial tonicity on the one hand, and the impulsive power of the heart on the other, are automatically compared. The finger is substituted by an instrument of precision, which replaces impressions by recorded facts self-analyzed. Dr. Anstie and Dr. Sanderson have for some time been pursuing a series of clinical and physiological observations by this instrument, and on this occasion Dr. Anstie discussed, by the aid of pulse-charts drawn to a large scale from the automatic register, some of the results of a clinical investigation by the aid of Marey's sphygmograph in the history of typhus and typhoid fever, and of the ephemeral fevers which occasionally simulate them. He seems to conclude that in both typhus and typhoid fever it appears to be the universal rule that the pulse is affected in a way which indicates great reduction of the arterial tonicity. This indication is given, on the application of the sphygmograph, by pulse-curves, which present a nearly vertical line of ascent, an acute summit, a sudden descent, and a very marked dicrotism, or double-beat. The pulse, thus strongly marked, continues, *at all hours of the day*, during the whole disease, up to the period of true convalescence. The prognosis seems to be always favourable as soon as the pulse has returned to that normal type which indicates the restoration of arterial tonicity, even though for some time it remain rapid. On the contrary, there appears to be always danger so long as the peculiar pulse remains. If, for instance, after the end of the third week in typhoid, or the second week in typhus, there is not a considerable alteration of the pulse-form in the direction of the normal type, dangerous sequelæ may be feared.

It contrasts with the peculiar type of pulse which, even from an early stage, distinguishes typhoid and typhus, the pulse in the fevers which we style "febricula" is not materially altered from the normal type, except as regards rapidity, even when much heat of skin and other decidedly febrile symptoms are present.

It is believed by Marey, and Dr. Anstie's experience so far seems to confirm it, that while typhus and typhoid fever (and also contagious erysipelas, purulent infection, low pneumonia, acute rheumatism, and delirium tremens) present the pulse of low arterial tonicity, the so-called true exanthemata—scarlatina, measles, small-pox, &c., show no such affection, at any rate in their early stage. This, if true, establishes a very important point in diagnosis, besides indirectly affording excellent suggestions for the pathological investigation of fevers.

These "points" are not stated in a dogmatic manner by Dr. Anstie. They are results which appear fairly to issue from an extensive investigation (more especially of typhoid fever.) But it is certain that the co-operation of a large number of careful and painstaking observers is necessary in order that the history of the pulse-form may be traced as completely as, for instance, Wunderlich has traced the history of temperature-changes in various fevers, which was only effected after 700 complete observations carefully conducted. At present, besides himself and Dr. Sanderson, to whom, indeed, the idea of the investigation was due, Dr. Anstie knew of none who has taken the matter up in this country. He determined, therefore, without loss of time, to direct the attention of English physicians to the immense field of fruitful observation which lies open to the investigation of observers who are willing to devote time and patience to the development of M. Marey's brilliant invention. —*Lancet, Nov.* 25, 1865, *p.* 599.

115.—THERAPEUTICAL NOTES.

There are symptoms met with in old persons which are often the precusors of apoplexy, and which remind one of those that frequently portend insanity, such as dizziness in the head, confusion of ideas, and general embarrassment of the mental faculties. Dr. Headland, in treating such cases in the out-patient department of the *Charing-cross Hospital*, finds that half-drachm doses of the solution of bichloride of mercury (L.P.) if given three times a day for three or four weeks, have a useful influence in removing these sensations. In the chronic bronchitis of the old, where there is great dyspnœa, accompanied by a dry state of tubes, he finds the tincture of stramonium, given in doses of ten minims, combined with other expectorants, very useful. In cases of excessive secretion, he finds no objection to the use of opium in half-drachm doses of the compound tincture of camphor. On the contrary, great relief results from the exhibition of this drug. In tænia, the result of his experience is that the oil of turpentine, combined with castor oil, three drachms of each, is a better remedy than the oil of male fern. The castor oil prevents those unpleasant head symptoms which are apt to arise if the turpentine is given alone. Turpentine he also finds very valuable in epilepsy. He gives it either in half-drachm doses three times a day, or in a single dose of three drachms at occasional intervals, combined with an equal quantity of castor oil. The results of treatment of lepra and psoriasis, with five-minim doses of the liquor arsencalis three times daily, and the external use of the liquid pitch ointment, he finds very satisfactory indeed. The arsenic can be continued for months without any deleterious effects. If such should arise, Dr. Headland suspends this medicine, and gives bicarbonate of potash for a short time, when the symptoms of poisoning—rash, irritation of the bowels, &c.—subside. In chorea, he has used nothing which has a better effect than iron. In chlorosis with suppressed menses, he finds the old Griffiths' mixture the best treatment, combined with half-ounce doses of the compound decoction of aloes in the morning.—*Lancet, Jan.* 6, 1866, *p.* 8.

116.—ON THE PHYSIOLOGICAL OPERATION OF THE PREPARATIONS OF IRON.

By Dr. A. Sasse.

Dr. Sasse, after making some remarks on the astringent properties of the preparations of iron, used both externally and internally, proceeds to discuss their chemical operation on the body. He considers them to be, like the blood-corpuscles, carriers of ozone, and therefore capable of replacing the corpuscles. These bodies change the oxygen taken into the blood by respiration into ozone, and the common oxygen of the atmosphere is just as useful for breathing as so much nitrogen; it must first be decomposed into the negative electric oxygen or ozone, and the positive electric oxygen or antozone, and the former operates as an immediately oxidizing agent. The ferruginous preparations only share with the blood-corpuscles this property of changing common oxygen into ozone, of giving off this ozone to substances inclined to oxidation, and of again taking up oxygen, until all are decomposed or eliminated from the body. The iodide of potassium paper is coloured blue when brought in contact with a solution of a persalt of iron, or with blood mixed with water. From this similarity in reaction, Schönbein has concluded that peroxide of iron is composed of protoxide of iron and ozone. Ferruginous preparations resemble the blood-corpuscles also in this respect—that they change the positive electric antozone into azone. Since the preparations of iron act altogether like the blood-corpuscles in relation to oxygen, ozone, and antozone, it will readily be understood that the former, when carried into the blood, may replace the loss of the corpuscles. It matters little whether the iron is taken into the blood in substance, or as an oxide, or as a proto- or persalt ; for when it has once entered the circulation, it continually takes up and gives off ozone, and thus becomes alternately a proto- and a persalt united, until it is expelled from the body.

It may perhaps be asked whether in the living body the ferruginous preparations act as oxidizing agents. Such is probably the case, and Botkin has shown that iron, even in healthy persons, raises the temperature above the normal standard. This elevation of temperature often occurs rapidly, and at the same time more urea is excreted, these phenomena all showing an increased oxidation. The same conclusion may be drawn from the similarity of the appearances observed (by Demarquay and Leconte) from the breathing of oxygen and from the operation of iron. Peroxide of hydrogen also, which contains antozone and has lately been recommended for the promotion of tissue-metamorphosis, may be compared with the preparations of iron, since antozone can be easily converted into ozone. Since also iron increases the temperature of the blood, it stimulates the action of the heart.

Thus, concludes Dr. Sasse, we have in iron a means of improving the tone of the system and promoting the process of oxidation. As this metal, therefore favours the nutrition and the activity of the body, it is contra-indicated when the process of oxidation is already too energetic, as in inflammatory diseases, or when an increased flow of blood is apprehended. In tuberculosis of the lungs, according to the author, iron ought to be avoided, notwithstanding some recent recommendations to the contrary.—*British and Foreign Medico-Chirurgical Review, Jan.* 1866, *p.* 233.

117.—SINAPINE TISSUE, OR MUSTARD PAPER.

(Prepared by MR. COOPER, Chemist, Abingdon-terrace, Kensington, W.)

This is a thin, glazed, innocent-looking paper, but any one who touches it with the tip of the tongue, will soon become conscious that it is coated with something of a very pungent character. Wetted with cold water and applied to the skin, it produces in about ten minutes a good deal of superficial irritation. Between it and a mustard poultice there seems to us this difference, that its effects are more persistent, and not quite so circumscribed, and that the sensations it produces are not the same. In fact, if we had to give a name to it, we should rather have called it *capsicine* than sinapine tissue. Be this as it may, it is so extremely portable, convenient, and clean, that persons who are in the habit of using mustard poultices or other ready forms of counter-irritation—as for coughs, sore throat, deafness, &c.—may easily carry a supply in their card case. Probably a hot poultice *over* a bit of this paper would answer well in cases of pleurodymia, &c.—*Medical Times and Gazette, Feb.* 10, 1866, *p.* 162.

118.—ON THE SUBCUTANEOUS INJECTION OF MORPHIA AND ATROPINE.

By Dr. C. E. BROWN SEQUARD, F.R.S.

Six or seven years ago I was led by the knowledge of the antagonistic effects of morphia and atropine on the brain to inject these two agents together, so as to reap the benefit of the good effects of the one added to those of the other against pain. The doses I at first employed were half a grain of the sulphate of morphia to one-sixtieth of a grain of the sulphate of atropine. For reasons to be mentioned hereafter, I now employ from one-half to two-thirds of a grain of the sulphate of morphia with one-twentieth or one-twenty-fifth of a grain of the sulphate of atropine. Great advantages are obtained by this combination: 1, an addition of the good effects against pain of the two remedies; 2, the possibility of employing safely, or at least without great cerebral disturbance, large doses of these narcotics.—*Lancet, March* 10, 1866, *p.* 249.

119.—ON THE ABSORBING POWER FOR MEDICINES POSSESSED BY CERTAIN PARTS OF THE BODY.

By Dr. C. E. BROWN SEQUARD, F.R.S.

The interesting experiments of Dr. B. W. Richardson, by which he thought he had found a valuable mode of producing local anæsthesia, have led Dr. Aug. Waller to a very important discovery. He has found that certain substances, such as atropine, strychnine, morphine, and the tincture of aconite, when mixed with chloroform and applied on the skin, are absorbed very rapidly; while, if alcohol, instead of chloroform, is mixed with the above substances, absorption is much delayed or even altogether prevented. The possibility of introducing rapidly into the blood the most active narcotics and other remedies without having to perform the operation of hypodermic injection will prove of great value in those cases in which an operation, however slight, is much dreaded, and in cases in which it may be found necessary to keep the patient for a long time under the influence of the treatment (for instance, in tetanus, in hydrophobia, in persistent neuralgic pains, &c.)

Within the last ten or fifteen years several facts of importance as regards the selection of the proper surface of absorption of certain remedies have been clearly made out. I will only mention the following:—1. Prof. Bernard and other observers have ascertained that woorara is hardly absorbed by the mucous membranes of the mouth and stomach, while it is very quickly absorbed when put in contact with the subcutaneous cellular tissue. 2. Mr. W. S. Savory has ascertained that strychnine is absorbed so much quicker by the mucous membrane of the rectum than by the stomach, that a dose only one-fourth of another will act with greater energy in the rectum than the much larger dose in the stomach. 3. I have found that ointments of belladonna and opium employed against neuralgic and other uterine pains act with greater rapidity and much more benefit when pushed up on a small lint ball in the rectum than in the vagina, showing that absorption is more rapid by the mucous membrane of the rectum than by that of the vagina. Not long ago the method consisting in applications of narcotics on the skin, deprived of its epidermis by a blister, was rather thrown into the shade owing to the superiority of the hypodermic method. I protest, however, against the abandonment of a method which has the double advantage of counter-irritation and rapid absorption of a narcotic, and which may sometimes prove more useful than subcutaneous injections. In a case of crural neuralgia which had not been markedly benefited by hypodermic injections of morphia and atropine, I have obtained a quick amelioration and ultimately a cure from a series of blisters (by the hartshorn ointment) and applications of sulphate of morphia on the denuded dermis.

Of two other methods of administration I will only say a few words. One of them is the inhalation of pulverized fluids; the other the use of medicated cigars or cigarettes. Hyoscyamus, stramonium, belladonna, and many other remedies may be employed with advantage by one or other of these means, not only in asthma, but in hooping-cough, in laryngismus, and also in tetanus, in hysterical aphonia, &c.—*Lancet, March* 10, 1866, *p.* 249.

120.—CHLOROFORM AS A MEANS OF DETECTING BILE IN THE URINE.

A new method for the detection of bile in the urine is proposed by M. Cunisset, and is given fully in the January number of the Journal de Chimie Médicale. He places in a test-tube forty or fifty grammes of the urine to be examined, and adds to it four or five grammes of chloroform, and then shakes the mixture. If the urine contains bile it immediately assumes a fine yellow colour, and on allowing it to rest, the chloroform falls to the bottom of the tube, drawing with it the fatty matters of the urine coloured by the yellow biliary substance.—*Lancet, March* 3, 1866, *p.* 238.

121.—AN ANTIDOTE FOR PRUSSIC ACID.

An antidote for prussic acid has been proposed by Messrs. T. and H. Smith. It consists of magnesia and perchloride of iron, and its effect depends upon the formation of the compound known as Prussian blue. It is recommended to give the patient first the magnesia (one or two drachms), made into an emulsion, with water; and then to give, in water, a solution of sixteen minims of perchloride of iron, and twelve grains and a half of green vitriol.—*Lancet, Nov.* 25, 1866, *p.* 604.

122.—ON ALCOHOL.

By Dr. HENRY MUNROE, Hull.

[The question as to the action of strong alcohol on digestion is of less consequence that as to its action in a very dilute form, as it exists in a glass of pale ale. The following experiments of Dr. Munroe are very interesting and important.]

Not having had the advantage of making any experiments with pure gastric juice obtained from the stomach of a living human being, as in the case of Alexis St. Martin, who had an opening made in his stomach by a gunshot-wound,—or as in the case of the Esthonian peasant, Catharine Kütt, who had for three years a gastric fistula,—from which openings gastric juice was easily obtained—my experiments have all been performed with gastric juice artificially made, and with meat digested out of the body.

The following experiments, out of the many performed, seem to be the most conclusive.

Experiment 1.—Four ounces of gastric juice having been poured into a wide-mouthed phial, half-an ounce of raw beef, cup up into small angular pieces, was then introduced. The phial was placed for ten hours in a sand-bath at the heat of 100° Fahr., being every two hours briskly shaken to imitate the motions of the stomach. After the first two hours, the fluid became opaque and cloudy; the surfaces of the bits of beef assuming a milky white appearance. In about four hours, the muscular fibres of the beef on the surface, and at the angles especially, began to separate and loosen. In about six hours, the pieces of beef were much lessened in size, and the fluid presented a more cloudy appearance, like thin broth. In about eight hours from the shaking of the phial, the pieces of beef were easily broken into shreds of muscular fibre, floating about in the fluid. In about ten hours, these muscular fibres became softened down and were no longer discernible, having become completely dissolved, the mixture presenting all the characters of soup.

Experiment 2.—Four ounces of the artificial gastric juice were poured into another phial, and I introduced half-an ounce of raw beef also cut into small pieces. I then added two drachms of rectified spirits of wine, and then placed the phial in the sand-bath. After two hours, little or no alteration had taken place in the fluid, which seemed to have no action on the beef. In four hours, the fluid was very slightly opaque, the beef remaining the same in appearance. In six hours, a slight albuminous coating was seen on the surface of the beef. After eight hours, there was no change to the sight; but the pieces of beef *felt more solid* during the operation of shaking the bottle. In ten hours, there appeared only the slightest change upon the surface of the beef, the substance of it not having been acted upon at all. On the phial having been set aside to cool, there was seen at the bottom of it *a deposit of pepsine*, which was not observed at the beginning of the experiment, and which did not appear at the bottom of the phial used in Experiment No. 1.

Experiment 3.—Instead of making the gastric juice with four ounces of water, I used that quantity of Allsopp's bitter ale. Half-an-ounce of beef was introduced into the phial as in the other experiments, and the phial was placed in the same sand-bath. In two hours, there appeared little or no change in the fluid or beef. In four hours, the fluid was rendered slightly cloudy, and the surfaces of the beef slightly coated with albumen. In six hours, the muscular fibres of the beef seemed somewhat separating and loosened. In eight hours, no further change had taken place. In ten hours, the beef presented a similar appearance to the beef in the phial containing alcohol—very little acted upon by the gastric juice. The phial, on being set aside, and cooling, *a deposit of pepsine* appeared, as in the preceding experiment. On reference to the experiments performed out of the body by Dr. Beaumont, with the gastric juice obtained from the stomach of St. Martin, I find that the process of digestion was performed in somewhat less time. But when a piece of meat tied to a string was suspended in the stomach, it became completely digested, and quite disappeared in the short period of an hour and a-half, showing the difference of the result in point of time, between natural and artificial digestion.

That alcohol, even in a diluted form, has the peculiar power of interfering with the ordinary process of digestion, I cannot have a doubt. That alcohol, in a diluted form, has the power of precipitating the *whole* of the pepsine used in these and other experiments, I will not affirm; but that it has the power of diminishing the efficacy of the gastric juice as a solvent for food, is sufficiently illustrated in my experiments.

On reference to the experiments performed in the human stomach, I may observe that Dr. Beaumont caused St. Martin, after a dinner of wholesome food, which was digesting happily, to take a glass of gin. The digestive process was immediately arrested and suspended, until the spirit had been absorbed into the general system, when the digestion of the food was slowly renewed and tardily completed.

Many years ago, after I had been an abstainer from all alcoholic drinks for six months, I partook of half-a-pint of Allsopp's bitter ale at dinner. In less than an hour afterwards I felt tired, exhausted in mind and body, inactive, rather feverish, and inclined to sleep, with increased fulness at the pit of the stomach, such as I had not experienced during my term of alcoholic abstinence. This fulness of the stomach, accompanied with slight difficulty of breathing, from pressure on the diaphragm, did not abate for three or four hours after taking the meal; nor did I again feel hungry that day, although I took my usual amount of exercise. For four days I continued to take the ale at dinner, but always experienced afterwards the same fulness at the pit of the stomach, and other symptoms of arrested digestion. At the end of the fifth day, happily for me, a fit of the gout supervening, caused my experiment to be suspended, and negatived the idea that Allsopp's bitter ale was, in any way, a promotor of digestion.

After a similar abstinence, I partook, daily, of half-an ounce of spirits of wine diluted with three ounces water at dinner. I again experienced the same feelings of distress at the pit of the stomach, inactivity, sleepiness, &c., arising, I have no doubt, from suspended digestion.

After a similar abstinence, I partook of three or four glasses of wine at dinner, and suffered again from the same symptoms. I have also partaken of almost every variety of food, accompanied with half to a pint of bitter ale, and have always experienced the same feelings of arrested digestion. For eight years I suffered from repeated attacks of gout, arising, I believe, from indigestion occasioned by the use of alcoholic beverages; but for some years, under total and persistent abstinence, I have never suffered from any symptoms of imperfect digestion, and my old enemy—the gout—has entirely retreated.—*British Med. Journal, Dec.* 9, 1865, *p.* 618.

123.—THE COURSE OF A DOSE OF ALCOHOL THROUGH THE SYSTEM.

By Dr. H. Bence Jones, F.R.S.

It passes through the stomach, and in going through is to a small extent oxidised. The greater part escapes, and enters the circulation, acting there on the oxygen, fibrin, albumen, and blood globules to a small degree. In a few minutes a part passes, still being oxidised, into every secretion, as, for example, into the lungs, kidneys, liver; whilst another part passes into every other texture; and on each substance in each texture the alcohol produces its chemical action, still continually undergoing oxidation, so that in a few hours one part has entirely escaped, and another part has been entirely oxidised in the blood and textures. What has the alcohol during this time done? It has acted chemically on oxygen and on the textures with which it has been in contact. The first action has little to do with the strength of the alcohol. Weak or strong, it combines with oxygen; increased oxidation, increased circulation, increased secretion, increased effusion of lymph, altered nutrition result. The second action depends on the strength of the alcohol. Strong alcohol acts chemically on albumen, fibrin, cellular tissue, &c. It has a powerful attraction for water, and it shrivels up cellular tissue, hardens fibrin, and precipitates albumen by its chemical properties.

To no organs of the body is more alcohol taken than to the kidneys and liver, and in no organs are the mechanical results of its chemical actions more manifest and the consequences of the altered structure more apparent; and the increased oxidation, the increased flow of blood, the altered nutrition are identical in kind with the first actions of inflammation; and when the series of actions are repeated over and over again for years, the result is the same as might have been produced in a shorter time by an ordinary inflammation when no alcohol whatever had been taken.

Dr. Christison states that three-fourths of all the cases of Bright's disease which he saw were produced by the habitual long-continued abuse of drink. Very great occasional excess did not act so strongly as long-continued smaller excess.

No doubt the alcohol acts on the vessels and ducts; but not so powerfully as on the interstitial texture.—*Med. Times and Gazette, Jan.* 6, 1866, *p.* 1.

124.—ON A NEW AND READY MODE OF PRODUCING LOCAL ANÆSTHESIA.

By Dr. Benjamin W. Richardson, M.A., Senior Physician to the Royal Infirmary for Diseases of the Chest.

[Some years ago Dr. Richardson published a paper on "Voltaic Narcotism." He had the idea that local anæsthesia could be produced by voltaic electricity, and succeeded in the case of feeble subjects; but in persons of average strength this actually increases the molecular motion of the part experimented upon. Anæsthesia really means the temporary death of a part influenced, i.e., inertia in the molecules of the part. Dr. Arnott's plan of using extreme cold was certainly the first step towards the production of local anæsthesia, but a more ready and easier plan of application is desirable.]

When the toy for diffusing eau de cologne in fine vapour over the skin, in the form of spray - which some time ago found its way into our drawing-rooms—first came before me, it struck me at once that it might possibly be applied to the production of local anæsthesia; and I set to work to try its applicability in this respect. I was soon afterwards assisted largely in my labours by taking advantage of Siegle's apparatus, with the hand-ball spray-producer invented by my valued friend Dr. Andrew Clarke, and supplied by the manufacturers, Messrs. Krohne and Sesemann, of Whitechapel-road.

With this apparatus I set myself to determine the degree of cold that could be produced by the vaporisation of all the known volatile liquids, and I determined the fact that the intensity of the cold produced held a definite relationship to the boiling-point of the fluid used; the rule being that the lower the boiling point the greater was the amount of cold exhibited. In these inquiries I employed a very delicate thermometer, directing the spray upon the bulb from half an inch to an inch and a-half from the point of the jet. By these means I learnt that with rectified sulphuric ether I could bring down the thermometer within 10 degrees Fahr. of zero, and that by directing the jet on the skin

I could produce a certain definite and marked degree of local insensibility, but not sufficient for surgical purposes.

I next got Mr. Krohne to construct for me a hollow cylinder of thin metal, six inches long and three inches in diameter. In the circumference of this cylinder was a chamber one-eighth of an inch in diameter for containing ether. The ether communicated with a tube which was joined to an air-tube, as in Siegle's apparatus, and the centre of the cylinder was filled with ice and salt mixture. In this way the ether was reduced to zero, and when vaporised gave spray which brought down the thermometer six degrees below zero, and produced on the skin such entire insensibility that I could pass a needle through the part without sensation. On the 11th of December, 1865, I applied this process for the first time on the human subject for an operation. The patient was a lady, who required to have five front teeth extracted. I had previously administered chloroform to this lady for a tooth extraction, but the inhalation had produced so much irregularity in the action of the heart and other disagreeable symptoms, that I considered it inadvisable to repeat chloroform, and she herself was only too ready to give the local measure a trial. The extraction was performed by my friend Mr. Peter Matthews. On directing the ether spray first at a distance and then closely upon the gum over the first central incisor on the left side, we observed, at the end of fifty seconds, that the gum had become as white as the tooth itself, and quite insensible. I then directed the vapour upon the tooth for twenty or thirty seconds more, and on the patient intimating that she did not feel, I suggested to Mr. Matthews to proceed. He extracted a very firm tooth without the slightest expression of pain. The process being continued in the same manner, he extracted three other teeth with the forceps. The fourth gave way, and had to be removed by the lever; but in all cases the result was equally good. Not a drop of blood was lost; there was no painful reaction; and the healing process proceeded perfectly. Our patient, who was exceedingly intelligent, was specially requested to note every step of the operation, such as the applying of the forceps, the insertion of the blades beneath the gum, the loosening process, and the removal. She told us that in two of the extractions she felt nothing; that in one it seemed as though the jaw altogether were being pulled downwards, but without pain; that in another she was conscious of a kind of wrench or loosening but without pain, and that the introduction of the lever was attended with a momentary dull ache, just perceptible. On the whole, the process was quite as painless as when she took chloroform.

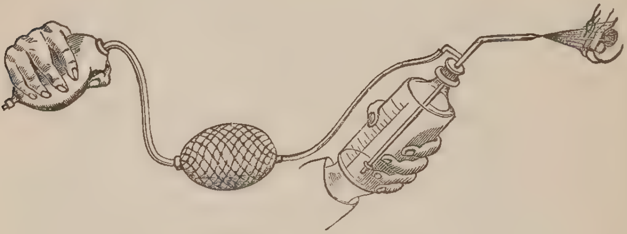
On December 13th I applied the local anæsthetic to the same lady for the further extraction of nine teeth, Mr. Peter Matthews again operating. The results were equally good with the first seven, at which point, unfortunately, the apparatus partly ceased play. At the eighth tooth pain was felt, and at the ninth, the apparatus being out of play, the operation caused great pain. We regretted this much, although it gave us the information of the perfect action of the process when no mechanical obstacle interfered with it. The reason why the apparatus stopped play was very singular, and could hardly have been foreseen. It arose from the condensation of water derived from the air in the air tube, and from the blocking up of the fine jet with a little portion of ice.

In the next step of research I got Mr. Krohne to make for me an apparatus with two spiral tubes, one the air tube, the other a tube for ether; and I immersed these spirals in a closed chamber filled with ice and salt. The degree of anæsthesia at first produced was most intense, and Mr. Spencer Wells was good enough to allow me the opportunity of applying the process in a case where an operation was required for closing a perineal rupture. Unhappily the apparatus, from the very same cause as before, ceased to yield a current; water condensed and became frozen in the air tube. The apparatus itself was also found to be too cumbersome for practical purposes; I therefore, in this trial, failed to obtain any result.

By this time I had been led, very reluctantly, to the fact that the use of ice and salt for reducing the ether was a failure when the plan came to be tried in practice, nor could I see any ready way of preventing the difficulties that were brought before me. Added to these difficulties there was another, which has always attended my friend Dr. Arnott's plan, viz., that of getting the ice and salt readily for operation. To succeed, therefore, it was requisite to dispense with ice and salt altogether.

In considering how this object could be achieved, it occurred to me that if a larger body of ether than is supplied by Siegle's apparatus could be brought through the same jet, by mechanical force, in the same interval of time, and with the same volume of air, a proportionate increase of cold must necessarily be produced. The theory was one of pure physics, admitting even of arithmetical demonstration, and running parallel with the lessons which had been taught me with respect to the cold produced by liquids having different degrees of boiling point. The theory was put to the test at once, and proved correct to the letter. By driving over the ether under atmospheric pressure, instead of trusting simply to capillary action—or to suction, as in Seigle's apparatus—the spray evolved brought the thermometer within thirty seconds to four degrees below zero—the result that was desired.

Ascertaining this truth, I instructed Messrs. Krohne and Sesemann to construct the very simple apparatus depicted below.



The apparatus consists simply of a graduated bottle for holding ether; through a perforated cork a double tube is inserted, one extremity of the inner part of which goes to the bottom of the bottle. Above the cork a little tube, connected with a hand bellows, pierces the outer part of the double tube, and communicates by means of the outer part, by a small aperture, with the interior of the bottle. The inner tube for delivering the ether runs upwards nearly to the extremity of the outer tube. Now, when the bellows are worked, a double current of air is produced, one current descending and pressing upon the ether forcing it along the inner tube, and the other ascending through the outer tube and playing upon the column of ether as it escapes through the fine jet. By having a series of jets to fit on the lower part of the inner tube, the volume of ether can be moderated at pleasure; and by having a double tube for the admission of air, and two pairs of hand bellows, the volume of ether and of air can be equally increased at pleasure, and with the production of a degree of cold six below zero.

By this simple apparatus, at any temperature of the day and at any season, the surgeon has thus in his hands a means for producing cold even six degrees below zero; and by directing the spray upon a half-inch test-tube containing water he can produce a column of ice in two minutes at most. Further, by this modification of Seigle's apparatus he can distribute fluids in the form of spray into any of the cavities of the body—into the bladder, for instance, by means of a spray catheter, or into the uterus by an uterine spray catheter.

When the ether spray thus produced is directed upon the outer skin, the skin is rendered insensible within a minute; but the effects do not end here. So soon as the skin is divided the ether begins to exert on the nervous filaments the double action of cold and of etherisation; so that the narcotism can be extended deeply to any desired extent. Pure rectified ether used in this manner is entirely negative; it causes no irritation, and may be applied to a deep wound, as I shall show, without any danger. I have applied it direct to the mucous membrane of my own eye, after first chilling the ball with the lid closed.

I have now employed this mode of producing local anæsthesia in four cases on the human subject. The first case was the extraction of a tooth from a lady, the operation being performed by my friend and neighbour Dr. Sedgwick, on January 24th of this year. On the 29th of the same month I used it again on the same lady for the extraction of three very difficult teeth, Dr. Sedgwick again operating. The results were as satisfactory as in the previous case, where the ice and salt ether apparatus was used.

I have used the apparatus also in connection with my friend Mr. Adams, who had a case at the Great Northern Hospital of deep dissecting abscess in the thigh of a young woman. In the abscess there was a small opening, which just admitted the director. I first narcotised around this opening, and the director being introduced, Mr. Adams carried his bistoury nearly an inch deep and one inch in the line of the direction. I then narcotised the deep-seated parts, and enabled him to cut for another inch and a half in the same direction. The director was then placed in the upper line of the abscess, the process was repeated, and the incision was carried two and a half inches in that direction. The patient was entirely unconscious of pain, and after narcotising the whole of the deep surface, Mr. Adams inserted his fingers and cleared out the wound without creating the slightest evidence of pain.

Afterwards, in the case of a lacerated wound, six inches long, in the arm of a boy, who had been injured with machinery, I narcotised while six sutures were introduced by Mr. Adams. The first needle was carried through without the anæsthetic, and caused expression of acute pain; the remaining eleven needles, after a few seconds' administration of the ether spray, were passed through painlessly. The twisting of the wire sutures gave no pain.

These results are so interesting that I make no apology for bringing them at once before my Medical brethren. I wish it to be distinctly understood that at the present moment I only introduce the method here described for the production of superficial local anæsthesia. It is, I believe, applicable to a large number of minor operations, for which the more dangerous agent chloroform is now commonly employed—I mean such operations as tooth extraction, tying nævi, tying piles, incising carbuncles, opening abscesses, putting in sutures, removing small tumours, removing the toe-nail, dividing tendons, operating for fistula, removing cancer of the lip, and other similar minor operations which I need not mention. The process may also be applied to reduce local inflammation.

In course of time, and guided by experience and the advancement of science, we may, however, expect more. If an anæsthetic fluid of negative qualities, as regards irritation of nerve, and which has a boiling point of 75° or 80°, can be obtained from the hydro-carbon series, the deepest anæsthesia may be produced, and even a limb may be amputated by this method. It may also turn out that certain anæsthetics may be added to the etherial solution with advantage, such as small quantities of chloroform, or some of the narcotic alkaloids, if they could be made soluble in ether. A solution of morphia and atropia combined, if they could be diffused through ether, which at present seems impossible, could thus be brought into action so as to cause deep insensibility. In operating on the extremities it would be good practice to stop the current of warm blood by making pressure above on the main artery.

Reaction from the anæsthesia is in no degree painful, and hemorrhage is almost entirely controlled during the anæsthesia.

One or two precautions are necessary. It is essential, in the first place, to use pure rectified ether; methylated ether causes irritation, and chloroform, unless largely diluted with ether—say one part in eight—does the same.

The *modus operandi* of this process is exceedingly simple. It acts at first merely by extracting force, and afterwards, when the nervous filaments are exposed, by preventing the conveyance of force through them. To be plain, sensation means the conveyance of force or motion from the extreme parts to the brain. The motion is communicated by the blood in the form of heat: it is communicated to the nervous filaments, and by them is conveyed to the sensorium. This is passive sensibility. When we irritate a nervous fibre, as by a cut, we communicate more motion rapidly along that fibre and cause pain. This is active or exalted sensibility. To remove sensibility, therefore, we must adopt one of three processes: we must remove or render inert the sensorium; we must stop the evolution of force generally by arresting oxidation of blood; or we must rob the body locally of its force beyond that with which it is constantly being renewed. We see the first of these processes in action in cases of pressure on the brain, as from injury or effusion of blood; we see the second whenever we produce general anæsthesia by charging the blood with chloroform or other analagous anæsthetic; and we see the third when, by means of extreme cold, we rob the local part of the force that has been brought to it by the blood.

The problem of local anæsthesia will consequently be quite solved when by a rapid process we can exhaust the natural force of a part as fast as such force is evolved in the local structure; and especially when with this we can combine the action of a substance which for the moment controls, as by compression, the conducting power of nerve matter. These two latter objects are to a large extent carried out by the method I have described above.

[The following paper contains one or two observations of importance made since the publication of the preceding article.]

I. I find that all the ordinary ethers of the shops contain alcohol, the presence of which substance materially interferes with the success of the process; it prevents perfect anæsthesia, and it causes tingling and burning sensation at the beginning of the process and during the brief period of reaction. I would, therefore, point out that before Practitioners resort to the process they test the purity of the ether they are about to use, by the following simple experiments :—

Tests for Ether.—(A) Take the specific gravity. The specific gravity should not exceed 0·723.

(B.) Try the boiling point. Warm the hands by gently blowing into them the warm breath. When the hands feel as warm as the breath, make the palm of one hand into a cup and pour in one or two drachms of ether. The ether ought immediately to boil briskly without giving any pain.

(C) Test the effect on mucous membrane. Put one or two drachms of the ether in the palm of the hand and quickly take up the ether into the mouth with the tongue. The ether should at once pass off, leaving neither smarting nor burning, nor any sensation except a slight coldness.

(D) Pour a little of the ether on a piece of clean white blotting-paper and lay the blotting-paper on the warm hand. The paper should dry within a minute, leaving no moisture and no smell whatever. If the paper, while drying, yield an odour like eau-de-cologne, there is some alcohol present. If it give a smell slightly pungent, and which hangs about for a time, there is some methylated compound present. Perfectly pure ether, in a word, leaves no persistent odour.

(E) Try the degree of cold producible by the ether. Charge the bottle connected with the spray producer, and direct the spray on the bulb of a thermometer. The mercury ought to fall rapidly to six degrees below zero Fahr., and the falling of the mercury should continue until there is a deposit of snow on the bulb of the thermometer from condensation of water in the air.

(F) Test the effect on the skin. Direct the spray, at a distance varying from half an inch to an inch and a-half from the jet, on the back of the hand. In a space of time, extending from thirty seconds to two minutes, a slight hoar-frost deposit should form on the skin, followed immediately by a diffuse blanching. The skin is at this moment altogether insensible.

(G) Test the reaction of the ether by litmus. The reaction should be neutral.

These are the ready and necessary tests. A pure ether answers to all of them, and no other ether ought to be used.

For a little time the Profession may experience a difficulty in obtaining pure ether, because it is not commonly vended. At the same time, as there is nothing more required than repeated rectification to render ether quite pure, it will be produced so soon as there is a demand ; and, although the prime cost will be increased, it will be saved in the end, because the quantity of ether required for each operation is reduced in proportion to the purity of the fluid. Already, indeed, Mr. Robbins, of the firm of Garden and Robbins, of Oxford-street, has, with his usual promptitude, produced for me an ether which bears all the tests I have described above.

II. I have observed in producing local anæsthesia by this new process that the peculiar hardness of the skin which occurs when the freezing mixture of ice and salt is applied, does not occur. The tissues, on the contrary are comparatively lax, so that difficult dissections may be carried on with ease and nicety.

III. I have found during the last few days that the application of the narcotic spray to the parts of the body subject to neuralgic pain gives immediate relief.

I have only to add, in conclusion, the gratifying intelligence that I have now applied the local anæsthetic in forty-three minor operations with a result of complete success in thirty-six cases, and with more than partial success in the remainder. In those cases where the success has not been absolute, the result has been due to imperfection either in the ether or apparatus, or to deficient experience, (unavoidable in all preliminary inquiries) in the mode of application. The reaction has been rapid, painless, and satisfactory.

[The most important element in the success of the plan of producing local anæsthesia introduced by Dr. Richardson is, that it supplies a ready means of removing pain arising from surgical operations or other causes, without the risk of sacrificing life.

For convenience, Dr. Richardson divides the following short paper under four heads.]

1. *The Principle of the Process.*—The principle of the new anæsthetic process consists in directing on a part of the body a volatile liquid having a boiling point at or below blood heat, in a state of fine subdivision or spray, such subdivision being produced by the action of air or other gaseous substance on the volatile liquid to be dispersed.

When the volatile fluid dispersed in the form of spray, falls on the human body, it comes with force into the most minute contact with the surface upon which it strikes. As a result there is rapid evaporation of the volatile fluid, and so great an evolution of heat force from the surface of the body struck, that the blood cannot supply the equivalent loss. The part consequently dies for the moment, and is insensible, as in death; but as the *vis a tergo* of the body is unaffected, the blood, so soon as the external reducing agency is withdrawn, quickly makes its way again through the dead parts, and restoration is immediate. The extreme rapidity of the action of this deadening process is the cause of its safety. The process can suspend life without causing disorganisation; if I may use the expression, it produces syncope of the part—temporary death—but not necessarily destruction. When we produce general anæsthesia we virtually extend this mere local action to the body altogether—*i.e.*, we check the evolution of force at the centre, and produce an approach to temporary death of the whole of the organism.

2. *The Instrument.—Improvements.*—The instrument by which the volatile fluid is dispersed is described at length in my paper published in this journal on February 3rd, [given above]. It consists of a spray-tube and bottle worked by Dr. Andrew Clark's hand bellows. The tube differs from all other spray-tubes in that the volatile fluid is brought up for dispersion by air pressure produced by the same motion as that which causes the dispersion. The instrument also provides a means for regulating the current of fluid; it allows the ether to be carried along tubes of any convenient length and curve; and, lastly, it enables us to construct a *compound* instrument by which the effects may be multiplied to any reasonable extent for large operations.

In my original paper, referred to above, I described simply the single dispersion-tube. Since then I have made a large number of tubes to answer various powers and purposes. I have a tube in which there is a bulb enlargement at the end with perforated side, or side and central jets. This tube is exceedingly useful for the cavities of the body, such as the vagina or rectum. It distributes the fluid in the same manner as a syringe with several perforations at its point. In practice, I find that the dispersion of the fluid delivered from one fine tube by a series of jets is not so efficient, proportionately, as when it is delivered by one jet: the fluid, that is to say, requires a certain degree of concentration to ensure success.

In order to multiply the anæsthetic producing power, I have other instruments constructed which may be called compound. In these cases the bottle holding the volatile fluid either receives a common central tube of large size communicating with a number of terminal jets, or each terminal jet has a separate jet running into the fluid. By this means I have a brush of jets, which may be circular, or long, or flat, as required. For this compound tube a six-ounce containing bottle for the fluid is necessary, and additional bellows power. The present small hand bellows will only work a compound jet of two elements with efficiency. I have tried to meet this difficulty by using a bellows worked by the foot, but not as yet to my full satisfaction. To get the air pressure I have tried various plans so as to do away with the hand bellows. I have used carbonic acid compressed into an iron bottle, and have applied the gas in its escape so as to act in the same manner as the air from the bellows. The apparatus complicates, and the pressure of gas cannot be nicely regulated. I have modified this plan also by trying to get force by generating carbonic acid gas at the time; also by generating hydrogen from zinc and dilute sulphuric acid, and using the pressure of the gas as the distributing agency.

Again, I have tried water pressure, as in the common gas holder; and I think in the dentists' room this plan would succeed well, if the preliminary expense were no obstacle. But taking all in all the hand ball bellows are as yet the most practical and most ready; they carry in the pocket, and one can go with them to the patient and commence anæsthesia at once—a great consideration. In many small operations, requiring only one or two strokes of the knife, the whole may be done painlessly, while the patient thinks that the preparations are merely being made—much, of course, to his gratification.

Several suggestions for the improvement of the jets offer themselves for consideration; the fish-tail gas-burner, the Argand burner, the conservatory water engine, and many other mechanical contrivances similar in kind will occur to every one as worthy of imitation, and as quickly as they can be made they will be produced and tested.

3. *The Fluid to be Used.*—I still continue to use absolute ether for operations, and now, as Mr. Robbins has produced an ether

of specific gravity 0·720 of negative effect on the tissues, and having a boiling point of 92° Fahr., a better fluid can hardly be demanded. Many other fluids have been suggested by the various readers of my original paper—viz., methylic ether, amylene, monochloretted chloride of ethyle, pure chloric ether, nitrite of ethyle, a volatile hydrocarbon derived from the manufacture of portable gas, chloroform, rectified turpentine, and numerous compounds and mixtures. As regards these I may state that they have all been under my careful consideration, but that as yet none of them, taking their qualities all in all, are equal to absolute ether. Some are open to rejection at once from their boiling point being too high; others are unpleasant, and would not admit of being used in operations on the mouth or teeth; others cause irritation of skin; others in their pure state are so extremely volatile that they could not be kept in the surgery for any length of time—this is specially the case with methylic ether and nitrite of ethyle, both promising substitutes for ether; lastly, a body too volatile would somewhat affect the operator during the operation if it were diffused in the pure state. Nitrite of ethyl is open to this objection not so much as the nitrite of amyl would be, but to some extent.

In time we may, perhaps, by experiment get a compound volatile mixture which being as negative as absolute ether in its effects on the body shall boil a few degrees lower.

Before leaving this topic, let me state that the mere alteration or change of the volatile fluid used is no change of the principle of the present anæsthetic process. Ether itself is only a local anæsthetic on being employed according to this principle. This is important to be borne in mind, otherwise a principle may become confounded with a detail, and every fluid with a low boiling-point and the other necessary physical qualities, as I have described them, for producing insensibility will be dubbed a local anæsthetic. By a slip of the pen, indeed, this error was committed in the Medical Times and Gazette of last week, a short leading article having been headed "Kerosolene a Local Anæsthetic." Kerosolene—a body very impure, unpleasant, and of unsteady but low boiling-point—when applied by my method acts like ether, because it resembles ether physically. But kerosolene is no anæsthetic *per se*, although it would serve as a substitution agent for one part of the anæsthetic process, in the same manner, and in none other, as an earthenware bottle would take the place of the graduated glass bottle in which the volatile liquid is commonly retained.

Dr. F. D. Fletcher, of Southport, has suggested to me the employment of liquid carbonic gas, and, as will be seen by last week's Medical Times and Gazette, Sir James Simpson has had carbonic in view for some years. I believe the first physiologist who actually tried to apply the reducing agency of carbonic acid for the production of anæsthesia was the late Dr. Snow. He went to great trouble and expense to experiment on the gas in the solid state, and he applied it in that state to his own skin. Singularly enough, the insensibility produced was slow and imperfect, but the worst feature was that a slough was always produced on the part where the acid had been applied. Snow, therefore, gave up the effort, convinced that carbonic acid in the solid form could never be made applicable in actual practice, and that if it could it would not be a safe agent.

I myself moved for a time in a similar direction by using carbonic acid in the liquid form. Mr. Robbins supplied me with the gas reduced in a three-pint iron bottle under pressure. When the stop-cock was opened and the carbonic was liberated through a fine jet, an intense cold was produced ; but I utterly failed in attaching a conveying tube that would be applicable for operations. The pressure, in a word, was unmanageable, and for ordinary practice, dangerous. In one experiment, while the jet was being directed on the back of my hand, the nozzle of the tube became set free, and, being projected with violence, injured me severely. I, therefore, like Snow, gave up carbonic acid as a body that would not submit to guidance, and as impossible to use in surgical practice.

We need not, fortunately, trouble ourselves on this subject. I have shown that by the dispersion of fluids of low boiling points we can get a degree of cold which answers the required purpose without employing fluids or gases under pressure. If we want more than absolute ether, chemistry can furnish us with fluids which boil even at below 70° Fahrenheit, which fluids, dispersed as vapour, would fill the purpose of carbonic acid with only one disadvantage—that of being difficult to keep in store during many months of the year.

4. *The Practice.*—[The Surgeon may produce either entire blanching of the surface to be operated on, or he may stop short of that extreme result, and only induce a superficial anæsthesia. The extreme action should be reserved for cases where deep-seated parts have to be divided.]

For producing the deep anæsthesia with superficial whiteness it is necessary to use absolute ether, and to direct the spray in brisk current at a distance of about an inch from the part. To induce the less determinate condition the ether may be diluted. This may be done by mixing alcohol with the ether, or better still chloroform. Two mixtures of this kind are very useful ; one contains six parts of ether and two of chloroform, the other seven of ether and one of chloroform. In using pure ether, or the mixture, differences of time are required. To cause insensibility with the simple fluid—ether—from fifteen to fifty seconds only are necessary. To produce insensibility by the mixture of ether and alcohol, or of ether and chloroform, from four to five minutes are demanded. The sensation felt by the patient also differs. When pure ether is used little if anything is felt until the moment when the part becomes white: then there is a sharp, pricking, burning sensation. When the compound or mixture is used, the sensation, very prolonged by comparison, is that of numbness and aching. On the whole, I have found patients generally prefer the more rapid procedure.

The nature of the operation will, to a large extent, determine the method to be resorted to. For opening an abscess, for incising a small carbuncle, for tying a nævus, for removing very small tumours, for applying nitric acid, and for operations of a similar kind, the mixture of ether and alcohol, or of ether and chloroform, answers every requirement. I should myself also use the mixture in an operation for hernia, because the tissues would not be rendered hard, and the dissections could be carried on with delicacy. But for deeper operations, such as removal of the nail, of portions of bone, of fingers, and the like, the complete action of the anæsthesia requires to be brought into play. For teeth extraction the pure ether also answers best—it acts rapidly and deeply, and there is no great accumulation of fluid in the mouth. By practice, the two degrees of action I have named may be obtained by the employment of ether alone: I mean, the degree of anæsthesia from the spray of absolute ether can be determined by the distance from the part at which the spray is directed: by removing the jet three inches from the part, a moderate effect is produced, nearly equivalent to the dilution of seven parts of ether with one of chloroform. The condition of the patient generally ought likewise to be considered. Aged and weak people become anæsthetic very readily, and for them the milder process is most applicable.

My researches on the production of local anæsthesia by means of ether spray have led me to invent a few new compounds of ether which cannot, I think, but prove useful in practice, and to which I would direct attention.

Hæmostatic Ethers.—In observing the influence of the cold produced by the dispersion of absolute ether during operations, nothing has struck me more than the effect of the cold in immediately stopping the flow of blood. For a time, cold alone, when carried to its fullest degree, prevents all venous and capillary hemorrhage, and even the hemorrhage from small arterial trunks. After a time, however, as reaction returns, and the vessels relax under the influence of heat derived from the renewed circulation, there is bleeding, which, if a wound be closed too quickly, is a cause of after trouble. The observation of the immediate effects of cold led me to think that if they could be supplemented by a styptic which would spray evenly with ether, and which would take up the constringing action when the vessels commenced to relax, an important desideratum in both Medical and Surgical practice would be supplied.

Xylo-Styptic Ether Spray.—With this object before me, I requested Mr. Robbins to make for me a solution consisting of absolute ether, having a boiling point of 92° Fah., charged to saturation at a low temperature with tannin, and afterwards treated with xyloidine, a little short of saturation. The compound, made with much care, came out well. It ran easily through the spray tube without blocking; it produced good local anæsthesia, and it possessed an agreeable odour.

In order to test to the extreme the effects of this preparation as a styptic, I took sheep's blood, removed all the fibrine previous to coagulation by whipping, and then let the blood remain exposed to the air for two days to ensure partial decomposition. In this way the blood was rendered nearly as fluid as port wine, and in the most unfavourable condition for being transformed into clot. A few drachms of this blood were now placed in a saucer, the saucer having been warmed to the temperature of the body. The spray of the styptic ether was then directed upon the blood from a full-sized spray tube, and in five seconds the whole mass of blood was so thoroughly solidified that the saucer could be turned upside down without any escape of fluid. The blood, which had previously presented the odour of putrefaction, was also deodorised, and remains quite inodorous at this date—ten days after experiment. The blood sets in a firm leathery consistence, covered on its upper surface with a fine whitish layer, with a bright vermilion colour beneath.

These are the effects of the styptic ether on blood, the spontaneous coagulability of which has been lost, and I had the pleasure of showing these effects at the College of Physicians on Friday last during a lecture on heat and cold in the treatment of disease; but these effects are trifling when compared with what takes place on blood newly drawn, and which contains fibrine. In this case the process of coagulation under the influence of the spray is the work, I had almost said, of a second.

When this spray is directed on an open bleeding living surface, the primary effects are those produced by the cold—namely, the condensation and whitening of the tissues. If blood be flowing, it solidifies, and when the parts relax, new blood that may ooze up enters the solid blood as though it were a sponge, quickly solidifying by coagulation and stopping further flow.

The applicability of this process for the arrest of hemorrhage will occur to the mind of every Practitioner. The substances used in the compound are innocuous, and the combined influence of the cold and the styptic are immediate, and so decisive that I can scarcely imagine any hemorrhage they would not control. I have not had an opportunity of testing the point, but I have no doubt from the influence of the styptic on the decomposing albumen of defibrinated blood that even in those cases of hemorrhage where the blood is preternaturally fluid, the styptic spray would arrest the hemorrhage entirely. Where the blood contains fibrine in a natural condition, I cannot imagine a case in which the fluid would not prevent exudation.

The essential elements of this process are three in number :

1. The immediate constringent effects of cold on the blood-vessels.

2. The chemical action of the solution on the fibrine and albumen of the blood.

3. The extreme mechanical fineness of distribution of the fluid on the bleeding surface.

The styptic ether can not only be applied to open wounds on the skin, but to hemorrhage after the extraction of teeth, and, by means of a uterine tube, to hemorrhage arising from cancerous disease of the uterus or other cause. It might also be applied to the rectum in cases of hemorrhage from piles.

The apparatus required for this styptic ether is mechanically the same as for ordinary ether—that is to say, my spray tube with Dr. Clarke's hand bellows. The tube, however, requires to be made of different metal from that ordinarily in use for local anæsthesia ; and I have therefore instructed Messrs. Krohne and Sesemann to construct a special tube for the purpose.

Ferro-Styptic Ether.—I have tried other experiments with the persalts of iron, which are more or less soluble in ether, especially the perchloride, and these one and all produced, as a styptic ether, rapid coagulation of blood. Solutions of iron salts in ether are not, however, more effective than the ether I have already described ; and as they destroy the tube rapidly, act upon clothing injuriously, and do not so thoroughly deodorise, I do not think they are in the main so practical.

The styptic ether, containing xyloidine and tannin, will keep ready for use any length of time, as there is nothing in it to undergo decomposition ; and as very small quantities of it are required ; it will become, I trust, of standard service to the Medical Practitioner. It would be of great use also to Surgeons on board ship, and particularly to Army Surgeons. In case of warfare it would be exceedingly useful on the battle field, as under the instruction of the Surgeon it could be used by an orderly, so as to prevent hemorrhage instantaneously in the case of flesh wounds. It would also form a useful addition to the Medical cabinet of travellers, who by necessity are removed from the direct succour afforded by Medical art.

I have invented some ether compounds—namely, a caustic ether, an iodised ethereal oil, and an ozonised ether, to which I will refer in a future number.—*Medical Times and Gazette, Feb.* 3, 17, *March* 10, 17, *and April* 28, 1866, *pp.* 115, 169, 249, 277, 439.

125.—RHIGOLENE, A PETROLEUM NAPHTHA FOR PRODUCING ANÆSTHESIA BY FREEZING.

By Dr. Henry J. Bigelow, Professor of Surgery in the Massachusetts Medical College.

The above name is proposed as convenient to designate a petroleum naphtha boiling at 70° F., one of the most volatile liquids obtained by the distillation of petroleum, and which has been applied to the production of cold by evaporation. It is a hydrocarbon, wholly destitute of oxygen, and is the lightest of all known liquids, having a specific gravity of 0·625. It has been shown that petroleum, vaporized and carefully condensed at different temperatures, offers a regular series of products which present more material differences than that of their degree of volatility, and that the present product is probably a combination of some of the known products of petroleum with those volatile and gaseous ones not yet fully examined, and to which this fluid owes its great volatility. A few of these combinations are already known in trade as benzolene, kerosene, kerosolene, gasolene, &c., all of them naphthas, but varying with different manufacturers. I procured, in 1861, a quantity of kerosolene of four different densities, and found the lightest of them, the boiling point of which was about 90°, to be an efficient anæsthetic by inhalation. When it was learned here that Dr. Richardson, of London, had produced a useful anæsthesia by freezing through the agency of ether vapor, reducing the temperature to 6° below zero, F., it occurred to me that a very volatile product of petroleum might be more sure to congeal the tissues, besides being far less expensive than ether. Mr. Merrill having, at my request, manufactured a liquid of which the boiling point was 70° F., it proved that the mercury was easily depressed by this agent to 19° below zero, and that the skin could be with certainty frozen hard in five or ten seconds. A lower temperature might doubtless be produced, were it not for the ice which surrounds the bulb of the thermometer. This result may be approximately effected by the common and familiar "spray producer," the concentric tubes of Dr. Richardson not being absolutely necessary to congeal the tissues with the rhigolene, as in his experiments with common ether. I have for convenience used a glass phial, through the cork of which passes a metal tube for the fluid, the air-tube being outside, and bent at its extremity so as to meet the fluid-tube at right angles, at some distance from the neck of the bottle. Air is not admitted to the bottle, as in Dr. Richardson's apparatus, the vapor of the rhigolene generated by the warmth of the hand applied externally being sufficient to prevent a vacuum and to ensure its free delivery; 15° below zero is easily produced by this apparatus. The bottle when not in use, should be kept tightly corked, a precaution by no means superfluous, as the liquid readily loses its more volatile parts by evaporation, leaving a denser and consequently less efficient residue. In this, and in several more expensive forms of apparatus in metal, both with and without the concentric tubes, I have found the sizes of 72 and 78 of Stubs's steel wire gauge to work well for the air and fluid orifices respectively; and it may be added that metal points reduced to sharp edges are preferable to glass, which, by its non-conducting properties, allows the orifices to become obstructed by frozen aqueous vapor.

Freezing by rhigolene is far more sure than by ether, as suggested by Dr. Richardson, inasmuch as common ether, boiling only at about 96° instead of 70°, often fails to produce an adequate degree of cold. The rhigolene is more convenient and more easily controlled than the freezing mixtures hitherto employed. Being quick in its action, inexpensive, and comparatively odorless, it will supersede general or local anæsthesia by ether or chloroform for small operations and in private houses. The opening of felons and other abscesses, the removal of small tumours, small incisions, excisions, and evulsions, and perhaps the extraction of teeth, may be thus effected with admirable ease and certainty; and for these purposes surgeons will use it, as also, perhaps, for the relief of neuralgia, chronic rheumatism, &c., and as a styptic, and for the destruction by freezing of erectile and other growths. But for large operations it is obviously less convenient than general anæsthesia, and will never supersede it. Applied to the skin, a first degree of congelation is evanescent; if protracted longer, it is followed by redness and desquamation, which may be possibly averted by the local bleeding of an incision; but if continued or used on a large scale, the dangers of frost-bite and mortification must be imminent.

It may be superfluous to add that both the liquid and the vapor of rhigolene are highly inflammable.—*Boston Medical and Surgical Journal, April* 19, 1866, p. 1.

125.—ON ANÆSTHESIA BY MIXED VAPOURS.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

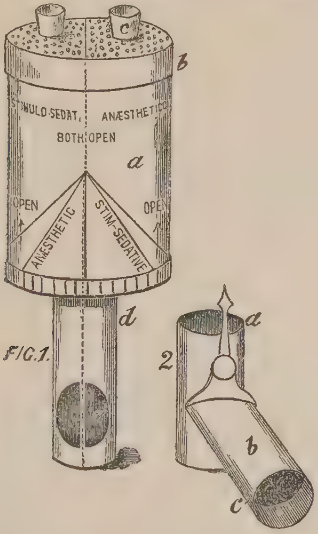
While engaged in obstetric practice it has long occurred to me how great would be the advantage of possessing a substance safe in the use, and at the same time capable of producing insensibility to pain. Such an agent is yet to be discovered. But I think I have found a method by which the same result may be obtained by a combination, in a certain manner, of those substances with which we are already familiar. Reflecting on the commoner causes of fatal chloroform accidents, the conviction is impressed on me that we commence our anæsthetic induction with the wrong agency. Chloroform, however diluted, is unsuitable for the *early stages* of inhalation, and it is in these especially that fatal results have been most frequent. At this period the emotional causes of danger are most active, the resistance greatest, and the danger also greatest. But even when judiciously administered, chloroform, in my judgment is, the wrong thing to begin with. Its physiological action as a stimulant is brief, while the narcosis which follows is prolonged. Also, notwithstanding many assertions to the contrary, I think there is evidence to prove that occasionally chloroform vapour acts as a depressant from the first. What it seems to me we require, therefore, is to commence the anæsthetic operation with some of those substances which will give a gentle stimulus to the patient, sustaining the heart-power, tranquillizing the emotional condition, and thus gradually introducing to the stage of first partial, and then complete insensibility. In the method I would now introduce, this is effected by a means so simple and secure, that I have good hope it may commend itself to the judgment of many of our profession.

I take a drachm of *pure and strong alcohol*, sp. gr. 810 to 815, and pour into one part of the instrument described below; this will last for four or five minutes. Into another division I pour a drachm of *pure ether*, sp. gr. 725, which lasts, perhaps, a couple of minutes; and I keep supplying this in small quantities as required. The patient begins to inhale, and receives into his lungs a mixed vapour of alcohol and ether, the evaporating surfaces being so adjusted as to keep a tolerable equilibrium between these two ingredients. By the simple act of rotating the apparatus, the quantity inhaled is increased, and is measured off on a scale to which the index points. The operator thus sees at a glance what amount is passing into the lungs, and adjusts it to the requirements of the patient. There is no coughing, or struggling, or resistance at this stage. Very gradually the pulse begins to rise, the surface becomes warm, (it is often cold from mere fright,) and the patient comes under the full influence of the mixed vapours; this I have called the stimulo-sedative stage of the operation. *Then*, when the heart and the nervous system have fully experienced the power of this excitant, a little turn further brings in the chloroform vapour, by such gentle gradations that the patient passes insensibly from the milder influence of the one agent into the anæsthetic power of the other; and by still further rotating the instrument, he is made to breathe only chloroform vapour just at the time when he is ready for the further proceeding of the surgeon. I think that in this method of inducing anæsthesia are combined the conditions of the greatest security with those of complete abolition of pain. I would urge upon my professional brethren to avail themselves of the principle which I here seek to bring before them, so simple in its nature that it has probably occurred to many of them before this. I would also help those who wish to put it, as I have done, into actual practice, by describing the instrument I have made, and used with success, and an engraving of which is appended. It is really a *working* instrument, without complication of parts, easily understood, quickly cleansed, and not very difficult to manufacture, my own having been made by myself out of a few bits of brass tubing, and then electro-plated.

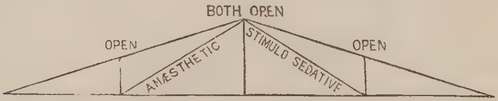
For prolonged anæsthesia, as in obstetric practice, I think this apparatus admirably adapted. After a partial anæsthetic effect is produced by the mixed vapours, it is then easy to turn on a little more chloroform with each wave of uterine effort, and to turn it off to the safer vapour back again as the "pain" subsides. By this means we obtain all the advantages claimed by the Chloroform Committee for their anæsthetic mixtures, with the additional one of having the potent agent most completely under our control. So far as I am able to judge, the use of the mixed vapour is followed by much less depression than sometimes ensues after choroform. It has also this advantage, that, whereas the use of chloroform alone (probably from its sweet, sickly odour) often leaves behind on the mind of the patient an excessive repugnance to the future use, or even the smell, of this substance, the same effect will not follow the employment of the vapours here recommended. In actual practice, I find it a slight good to dissolve a small quantity of some aromatic substances in the alcohol or ether, and none better can be had than the aromatics used in sal volatile—namely, the oil of lemons and the volatile oil of nutmegs. It is useful also to colour the fluids slightly, so that they may be at once recognised, and mistakes for chloroform prevented. But I must very carefully recommend the operator to make sure that he is using *pure absolute* ether and *absolute* alcohol, perfectly free from the abominable contamination of methyl and its compounds. By care in this respect he will secure constant uniformity in his operations, and avoid the ill effects of those adulterations to which alcohol and ether are too subject. It is also important to observe that the ether and alcohol are really absolute. For this purpose the ether should be washed in distilled water, and then drawn off by a water-bath. The strongest commercial alcohol should also be procured and redistilled from chloride of calcium, drawing off three-fourths of the amount held by the still. The advantages are these, that definite fluids of known strength are employed, the results of the use of which will always be comparable with each other, and the inhaler does not become charged with water or the heavier parts of the spirit. By proceeding thus, a highly stimulating, easily vapourisable, and a very agreeably smelling spirit is obtained—very different from the ether and alcohol of the shops.

The question will be asked as to the value of using these anæsthetics in a separate form, when all can be had combined in the mixtures so strongly recommended by the Chloroform Committee. The principal and most important advantages are these: that we thus isolate and have under our control a perfectly safe stimulant and mild anæsthetic, while in the same instrument we have at command a most valued and powerful agent for the abolition of pain. And these are capable of being so united and blended together in any desired quantities as to give us such control over the individual case operated on as that may seem to require. We are able to produce any degree of anæsthesia by this means, from the slight dreamy unconsciousness to the deepest coma; and by aid of the index we can at any moment observe what is the precise amount of either or of both agents which is being employed. Anæsthesia may, in fact, be procured more quickly than by any of the anæsthetic mixtures, and it may then be graduated to the varying condition of the individual—in the paroxysm of labour pain and in the interval of comparative freedom, in the division of skin and sensitive structures, and in the dissection of less nerve-endowed parts. Another and most important advantage is, that whereas anæsthetic mixtures all vary in composition by prolonged employment, and clog the instrument with unevaporated heavy fluid, no such effect occurs in this method.

For the mouth piece I employ Snow's, or any other with a similar valve arrangement. The part of my instrument which is peculiar is the receptacle for chloroform and the corresponding one for the alcohol and ether. This part consists of two separate pieces, an upper and a lower (Figs. 1 and 2); the upper fitting as in a socket into the vertical part of the lower tube. The upper part consists of a tube (*a*) of a little more than two inches diameter, and is three in height. It is fitted with a perforated lid (*b*), at each side of which is a small funnel (*c*). This tube is divided through its whole length by a metal diaphragm (*e*), which also runs down into the smaller tube fitted into it at the bottom. The whole forms one compact piece from top to bottom, and enclosing two entirely distinct spaces, one of which is reserved for the chloroform, and the other for the alcohol and ether. These have no communication with each other, but have a common opening of an elliptical figure at the bottom (*e*) of the smaller tube. This smaller tube (*d*) is accurately fitted, so as to work easily round in the vertical piece of the other part (Fig. 2) of the instrument. In so doing it necessarily exposes, according to the direction in which the larger tube is turned, first one side of the opening (say that leading to the alcohol and ether compartment), and then the other, leading to the chloroform. It is thus seen that we can perfectly control the nature of the vapour allowed to pass into the other part of the instrument, according as we cause it to turn to the right or to the left,



The second part (Fig. 2) of my instrument forms the receptacle for the socket (*a*) of the first, and carries the vapour up to the mouth-piece. But in order thoroughly to mix the separate vapours before they are breathed, a piece (*c*) of perforated gilt metal is fixed inside the horizontal arm *(b)* of this piece, and the diffusion thus caused effects a complete mixture before the vapours rise to the mouth. This same horizontal arm carries the index-finger, which points to the scale engraved at the upper part of the receptacle, and by its means (the scale having been carefully adjusted by experiment to the openings below) the quantity and the quality of the vapour are at once read off. What is told by this scale is represented below in the diagram. The rise of the lines exhibits the gradual increase of aperture. The graduated marks answer the double purpose of an index and a safeguard; for the index-finger fits into them with a spring, and holds the apparatus at the required degree until it be desirable to shift it farther. The instrument may thus be consigned to a nurse for a time, and the operator feel secure that no excess of chloroform can be accidentally administered.



[In a second paper published by Mr. Ellis on this subject, he says that during the three months which have elapsed since the first article was written, he has used the apparatus in a number of instances, and has added to it several improvements.]

The fluids I employ are—1. *Anæsthetic alcohol.* This has a specific gravity of 800 at 60° Fahr., is free from methylated adulterations, and may be prepared by the process indicated in the British Pharmacopœia. 2. *Anæsthetic ether.* This is perfectly free from methyl, should have a specific gravity of 720, and may be prepared by well washing the best sulphuric ether in distilled water, and subsequently drawing it off by a gentle heat into a receiver. It should be used fresh, and possess an agreeable aromatic odour, free from any acrid or pungent character. 3. *Pure chloroform.* This should have a specific gravity of 1495, and be absolutely pure and free from all contamination, whether of methyl or chlorine. The Edinburgh chloroform is that which alone I have used. The alcohol and ether are slightly aromatised, and tinged with colour to distinguish them from each other and the chloroform.*

The principles of my system of anæsthesia are to make provision for—

1st. Entire security against the excessive action of either of the anæsthetics.

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* Messrs. Savory and Moore, of New Bond-street, have given much attention to the preparation of these fluids, and the products of their establishment may be thoroughly depended upon for excellence and purity.

2nd. The production of a modified anæsthesia, *varying, at the will of the operator*, from a mere feeling of exhilaration to the deep unconsciousness requisite for abolishing acute pain.

3rd. The power of maintaining this state of anæsthesia at the same degree for any requisite period, or of modifying it to arising exigencies.

4th. The reduction of the dose of chloroform to its lowest practicable point.

5th. The partial substitution for it of a vaporous basis of mixed alcohol and ether, whereby its properties are enhanced and sustained, and its dose diminished without abatement of its value as an anæsthetic.

6th. The counteraction of the heart-depressing power of chloroform, by combining it with a heart-stimulant, and thus obviating some of the most frequent causes of danger in chloroformization.

The apparatus by which I have perfectly succeeded in fulfilling these objects is identical in principle with that described in my original article, but is different in its detail and method of construction. It has been in its improved form extremely well manufactured from my designs by Messrs. Savigny, of St. James's-street, from whom it can be obtained.

My object having been chiefly to get a good arrangement for use in the lying-in chamber, I have devised the following instrument, which has proved really valuable. It consists of a metallic cylinder about three inches in height by two inches and three-quarters in diameter. On one side, near the upper part, the tube emerges which conveys the vapourized air from the chambers contained in the cylinder. This elbow tube has attached to it a valve for admitting air at the commencement of the process, and it is movable so as to adapt itself to the different positions of the patient. Penetrating the cylinder, it communicates with a small circular chamber, at the bottom of which are two apertures, one connected with the chloroform side, and the other with the alcohol and ether. The different currents of air charged with vapours are thus caused to come together at the bottom of this chamber, and the openings in it are covered by a circular valve perforated in one half, so as to correspond with these openings, and the other half being left blank. A little consideration will show the reader that if this flat plate be turned by a pin passing through its centre, it will, according to the direction of the rotation given to it by the pin, uncover either one or other of these openings, and at one point both are wide open. After a few respirations the air-valve is gradually closed, and the index finger is advanced by single degrees up to ten. This is the point where chloroform influence faintly commences. The index is now moved still more gradually until it marks seventeen or eighteen degrees on the scale. Of these degrees, ten represent the mixed vapour of alcohol and ether, and the remainder that of chloroform. I very seldom find it necessary to advance the index further than this point in a confinement; but for a surgical operation it will perhaps be necessary to go beyond it, and to give the vapour uninterruptedly from both sides of the apparatus; or if very deep anæsthesia be demanded, then the index must be taken a full half turn onward, so as to give the predominance to the chloroform, and render the alcohol and ether mere auxiliaries. It is satisfactory to know that it is but seldom that the full power of the instrument is really required, while at no point is it capable of giving a dangerous dose to the patient. Opposite to the elbow is a small space marked "off," indicating that when the finger is at that point, no vapour whatever can leave the apparatus. Thus it can be turned off in the intermission of the inspiration, and the waste of the fluids prevented.

Two perfectly novel and remarkable features of this arrangement are illustrated in the apparatus: first, the method of supplying the chloroform and ether, which by a simple adaptation of the laws of capillary attraction, *cannot* escape in any dose exceeding about 2½ per cent. and, secondly, the method of charging the air with alcohol vapour by an arrangement of cambric folds resembling the gills of a fish, and, indeed, designed from a consideration of those organs. By this means I have been enabled to give to the patient a larger proportion of alcohol in vapour than has ever been attempted hitherto, and with most excellent results.

Lastly, the economy of my method is most extraordinary. As a rule, in midwifery I have not consumed more than a *single drachm* of chloroform, and the same quantity of alcohol, and ether *per hour* of inhalation—intermittent, be it remembered, as it always is in obstetrics; and I believe from a drachm and a half to two drachms would be the extremest quantity necessary for any ordinary purpose. This may be well contrasted with the ounces of chloroform and the pints of ether hitherto deemed essential.

As I do not practise the art of chloroformization beyond the circle of my own professional domain, I have only the wish to make known a valuable addition to our art in the publication of this and the former article. I shall therefore feel honoured and happy if anyone with more leisure than myself will take up my views and pursue them to their widest application.—*Lancet, Feb.* 10, *and May* 12, 1866, *pp.* 145, 509.

INDEX TO VOL. LIII.

PAGE.
Absorbing power for medicines of certain parts of the body 363
Acetic acid, its use in cancer 330
Acids, Dr. Bence Jones on the action of xxxvi
Actual cautery, Mr. Baker Brown on securing the pedicle by in ovariotomy 300
——— cautery, Mr Douglas on its use for nævi 200
Acupressure, Mr. Macgregor on the value of 42
Acute orchitis, Dr. Watson on the treatment of 198
—— rheumatism, Mr. Macgregor on the efficacy of blistering in 42
—— rheumatism, Mr. Moore on the efficacy of blistering in 43
Adams, Mr. W., on subacute synovitis and contracted joints 163
Albuminuria from imperfect digestion, Dr. Harley on 138
———— from reflex irritation, Dr. Harley on 139
———— of pregnancy, Dr. Harley on 139
Alcohol, Dr. Bence Jones on the course of a dose of through the system 268
——— Dr. Munroe on its action on digestion 365
Alcoholic stimulants in typhus fever, Dr. Perry on the use of 31
Alkaline urine, Dr. Bence Jones on the symptoms arising from 131
Althaus, Dr. J., on progressive locomotor ataxy 51
Ammoniacal urine, Dr. Bence Jones on the treatment of 132
Anchylosis of the knee in the straight position, Dr. Smyly on forcible flexion in 162
Anderson, Dr. K., on the daily execretion of urea in typhus fever 32
Anæsthesia by mixed vapours, Mr. R. Ellis on 386
———— local, Dr. Bigelow on rhigolene for inducing 384
———— local, Dr. Richardson's ether-spray method of inducing 369
Aneurism, Dr. Duncan on the galvano-puncture of 180
———— Prof. Syme on ligature of the femoral artery 189
———— popliteal, Mr. Hilton on the treatment of 191
Apoplexy, Dr. Headland's treatment of the precursory symptoms of 360
Ataxy, progressive locomotor, Dr. Althaus on a case of 53
——— progressive locomotor, Dr. Jackson's case of 59
——— progressive locomotor, Dr. Johnson's case of 58
Atropia, Dr Fleming on its use in constipation 111
Atropine points, Mr. Laurence on 212
Aveling, Dr. J. H., his case of ovariotomy 294

Barclay, Dr. J., on citric, acetic, and carbolic acids in cancer 329
Barnes, Dr. R., on dysmenorrhœa and its treatment 301
Beck, Dr. S., on enlargements of the uterus following abortions &c. 251
———— on puerperal fever 241
Bed-guard and leg-suspender, Mr. Greenway's 172
Beigel, Dr. H., on the treatment of pruritus senilis 202
Belladonna and bromide of potassium in epilepsy 73
Bigelow, Dr. H. J., on rhigolene for procuring local anæsthesia 342
Bile in urine, M. Cunisset on chloroform as a means of detecting 365
Bishop, Dr. E., his instrument for applying remedial agents to cavity of tympanum 216
Black cataract, Mr. Walton on the extraction of 209
Black, Dr. D. C., on an outbreak of typhoid fever 37
Blistering in acute rheumatism 42
Blue glasses, Prof. Bohm's preference for 212
Bowels, obstruction of, Dr. Fleming's case successfully treated by stomach-pump 357
Broadbent, Dr., on use of belladonna and bromide of potassium in epilepsy 73

PAGE.
Bright's disease, Dr. Harley on the treatment of 144
Bromide of ammonium in the sleeplessness of functional nervous diseases .. 73
——— of potassium in epilepsy 73
Bronchitis, chronic, of the aged, treatment of 361
Brown-Sequard, Dr. C. E., on functional nervous affections 67
——— on absorbing power for medicines of certain parts of the body 363
——— on the moral treatment of functional nervous diseases .. 72
——— on subcutaneous injection of morphia and atropine .. 363
——— on the treatment of chlorosis 314
——— on the treatment of epilepsy 75
——— treatment of sleeplessness in nervous diseases 73
——— on the use of chloroform in epilepsy and hysteria 79
Brown, Mr. I. B., on the actual cautery for securing the pedicle in ovariotomy 300
Brunton, Dr J., his new otoscope, or speculum auris 214
Buchanan, Dr. G , on tracheotomy in croup and diphtheria.. 120

Calabar bean, Dr. Ogle on its use in chorea.. 83
Cancer, Dr. Barclay on citric, acetic, and carbolic acids in 329
——— Mr. De Morgan on, and its treatment 147
——— Mr. Denny on citric acid as an application in 44
——— of the breast, Mr. Moore's case of 151
Cancerous tumour of the arm, Mr. Hilton's case of removal of 45
Carbolic acid, Dr. Wolfe on its use in phthisis 108
——— acid, its efficacy in the treatment of cancer 330
Cataract, black, Mr. Walton's case of extraction of 209
Caustic applications, Dr. Routh on the treatment of prolapsus uteri by .. 264
Citric acid as an application in cancer 44
——— its use in cancer 329
Chambers, Dr. T. K., on the treatment of typhoid fever 40
Chapman, Dr. J., on the use of ice-bags in convulsions and in cholera .. 80
Chilblains, Dr Purdon on the nature and treatment of 201
Childbed, Dr. Matthews Duncan on the mortality of 323
Chlorate of potass in ovarian disease, Mr. Craig on 296
Chloride of zinc in surgical operations and injuries, Mr. De Morgan on .. 147
——— of zinc lotion in the treatment of cancer, Mr. De Morgan on .. 151
Chlorocarbon, Dr. Sansom on its use in midwifery xxx
Chloroform and ether, Dr. Sansom on xxxvi
——— Drs. Spence and Gillespie on prevention of danger from xxxvi
——— its use in epilepsy and hysteria 79
Chlorosis, Dr. Brown-Sequard on the treatment of 314
——— with suppressed menses, Dr. Headland's treatment of 361
Cholera, Dr. Bowerbank on the saline treatment of xix
——— Dr. Chapman on the use of ice-bags in 80
——— Dr. Gason on the treatment of xix
Choleraic diarrhœa, Dr. Johnson on the treatment of xix
Chorea, Dr. Headland on the use of iron in 361
——— Dr. Ogle's treatment of by the Calabar bean 83
Chronic bronchitis of the aged, Dr. Headland's treatment of 361
Coates, Mr. W. M., his case of glandular tumour of mamma with cysts .. 315
——— his case of large ranula treated by injection of iodine .. 316
——— his case of spina bifida treated by injection of iodide of potassium 317
Colles's fracture, on its treatment by Dr. Gordon's splint 166
Congestion, Dr. Merril on the internal use of chloroform in xvi
Constipation, Dr. Fleming on the use of atropia and galvanism in 109
Contracted joints, Mr. Adams on the treatment of 163
Convalescence from inflammation, use of nitric acid in 128
Convulsions, on the use of ice-bags in 80
Cotton, Dr. R. P., on hemoptysis unassociated with pulmonary tuberculosis .. 105
Craig Mr. W., on chlorate of potass in ovarian disease 296
Croup, Dr. Buchanan on tracheotomy in 120
Cunisset, M., on chloroform for detecting bile in urine 365

PAGE.
De Morgan, Mr. C., on chloride of zinc in surgical operations and injuries 147
Denny, Mr. C. J., on citric acid as an application in cancer 44
Detached placenta, Mr. Tubbs's case of hemorrhage from 253
Diabetes insipidus, treated by large doses of valerian 48
Difficult turning, Dr. Heyderdahl on the use of wire-ribbon in 248
Digestion, Dr. Munroe on the action of alcohol on 365
Dilatation of the uterus, Mr. Hembrough's new instrument for 286
Diphtheria, Dr. Buchanan on tracheotomy in 120
——— Mr. Maynard on the use of hyposulphite of soda in 119
——— complication with enteric fever 39
Disease, Dr. Tilbury Fox on the action of fungi in the production of 343
Dislocation of the shoulder-joint, Mr. Porter's easy method of reducing 164
Displacements of the uterus, Dr. Main's support for 275
——— of the uterus, Dr. Wright on 279
Dobell, Dr. H., on the nature, cause, and treatment of tuberculosis 36
Douglas, Mr. J. R. A., on the actual cautery for nævi 200
Drowning, resuscitation after, Mr. Ellis's case of 342
Duncan, Dr. J. M., on mortality of childbed as affected by number of the labour 323
Duncan, Dr. J., on the galvano-puncture of aneurism 180
Dysmenorrhœa, Dr. Barnes on the treatment of 301
——— Dr. Greenhalgh on the operation for 311
——— Dr. Routh on the operation for 312
——— Dr. Savage on 312
——— Mr. Baker Brown on the operation for 311
Dyspepsia associated with chronic affections, M. Trousseau on hydrochloric acid in 129

Eczema, acute, Mr. Johnston on the treatment of 203
——— infantum, Mr. Wilson's treatment of 203
Ellis, Mr. R, on anæsthesia by mixed vapours 386
——— on the radical cure of prolapsus uteri 260
Endemic or miasmatic fevers, use of the sulphites and hyposulphites in 335
Enlargements of the uterus following abortions and confinements, Dr. Beck on 251
Epilepsy, Dr. Broadbent on use of belladonna and bromide of potassium in 73
——— Dr. Brown-Sequard on the treatment of 75
——— Dr. Brown-Sequard on the use of chloroform in 79
——— Dr. Headland on the use of turpentine in 361
Erysipelas, Mr. Hilton on the treatment of 45
Ether, Dr. Richardson's tests for purity of 375
——— spray apparatus for inducing local anæsthesia, Dr. Richardson's 372
——— xylo-styptic, Dr. Richardson on 382

Femoral artery, Prof. Syme on ligature of the 189
Fergusson Sir W., on recurring fibroid disease of the shoulder 177
Fever, Dr. Bence Jones on 1
——— Editor of Lancet on the value of the thermometer in 7
——— enteric, complication of diphtheria with 39
——— infantile, Mr. Smith's thermometrical observations in 11
——— mechanical disorders that arise out of the chemical errors in 4
——— puerperal, Dr. Snow Beck on 241
——— typhoid, Dr. Black on an outbreak of 37
——— typhoid, Dr. Chambers on the treatment of 40
——— typhus, Dr. Anderson on the excretion of urea in 32
——— typhus, Dr. Perry on the treatment of 29
——— typhus, Mr. Randolph on 35
Fibroid disease of the shoulder, Sir W. Fergusson on recurring 177
Fleming, Dr. A., on the treatment of constipation and stoppage of the bowels 109
Fleming, Dr. E., his case of obstruction of the bowels 357
Flexions of the uterus, Dr. Wright on extreme 279
Flour of meat, Dr. Hassall's 333
Fœtus retained in abdomen 43 years, Mr. Watkins's case of 249

PAGE.
Food, Liebig's, for infants and invalids 326
Fox, Dr. T., on the action of fungi in the production of disease 343
—— on wheat phosphates 130
Fracture apparatus, Mr. Greenway's 173
—— of the humerus, Mr. Hutchinson on 168
—— of the radius at styloid process, Mr. Tait on use of Gordon's splint in 165
Functional nervous affections, Dr. Brown-Sequard on 67
—— —— nervous diseases, Dr. Brown-Sequard on the moral treatment of .. 72
—— —— nervous diseases, use of bromide of ammonium in the sleeplessness of 73
Fundus oculi, Dr. Roseburgh's method of examining 204
Fungi, Dr. Tilbury Fox on its action in the production of disease 343

Galvano-puncture of aneurism, Dr. Duncan on the 180
Gibson, Dr. F. W., on the thermometer in the diagnosis of pyrexial diseases .. 20
Glandular tumour of mamma with cysts, Mr. Coates on injection of iodine in.. 315
Gonorrhœa and gleet, Mr. Henry Thompson on soluble bougies in xxvii
Goolden, Dr. R. H., on the treatment of disease by oxygen 49
Granular ophthalmia, Dr. Stokes on the treatment of 204
Greenway, Mr. H., his new form of leg-suspender and bed-guard 172

Harley, Dr. G., on secondary albuminuria 138
—— —— on the treatment of Bright's disease 144
Hart, Mr. E., on the treatment of gonorrhœal and purulent ophthalmia .. 208
Hassall, Dr A. H., on the concentration and preservation of meat 339
Hassall's flour of meat 338
Headland, Dr., his therapeutical notes on various subjects 361
Hembrough, Mr. J. W., his new uterine dilator 286
Hemoptysis unassociated with pulmonary tuberculosis, Dr. Cotton on .. 105
Hemorrhage from detached placenta, Mr. Tubbs's case of 253
Hepatic disease in India, Sir Ranald Martin's treatment of 125
Heyderdahl, Dr. V., on the use of wire-ribbon in cases of difficult turning .. 246
Hilton, Mr., his case of wounded ulnar artery, sloughing and secondary hemorrhage 194
—— on removal of a cancerous tumour of the arm 45
—— on the treatment of popliteal aneurism 191
Holthouse, Mr. C., on the treatment of strabismus without operation .. 210
Humerus, Mr. Hutchinson on some injuries to the head and neck of .. 168
Hutchinson, Mr. J., on some injuries to the head and neck of the humerus .. 168
Hyposulphite of soda in diphtheria, Mr. Maynard on the use of 119
Hysteralgia, Sir J. Y. Simpson on chlorocarbon vapour in xxxii
Hysteria, Dr. Brown-Sequard on the use of chloroform in 79

Ice-bags, Dr. Chapman on their use in convulsions and in cholera 80
Infant's food, Dr. Fox on the desirable properties of wheat phosphates for .. 131
Inhaler for producing anæsthesia by mixed vapours, Mr. Ellis's .. . 389
Interstitial nephritis, Dr. Bence Jones on 134
Intestinal obstruction, Dr. Fleming on the use of atropia in 109
Iodide of potassium, case of spina bifida cured by injection of 317
Iodine, case of glandular mammary tumour treated by injection of 315
—— case of large ranula cured by injection of 316
Iritis, Mr. Teale, jun. on the use of atropine in xxix
Iron, Dr. Sasse on the physiological operation of preparations of 361

Jackson, Dr. H., his case of progressive locomotor ataxy 59
Johnson, Dr. G., his case of progressive locomotor ataxy 58
Jones, Dr. C. H., on spasm 61
Jones, Dr. H. B., on interstitial nephritis 134
—— —— on fevers 1
—— —— on the course of a dose of alcohol through the system .. 368

PAGE.
Jones, Dr. H. B., on the treatment of ammoniacal urine 132
——— on tubular nephritis 137
——— on the symptoms arising from alkaline urine 131

Knee-joint, Dr. Smyly on forcible flexion in anchylosis in the straight position 162
——— Mr. Adams on the treatment of effusion into the 163

Labordette, Dr, his new bivalve speculum for the larynx 100
Lane, Mr. J. R., on vaginal rectocele 287
—— on syphilization 237
Larynx, Dr. Labordette's new bivalve speculum for 100
Laurence, Mr. J. R., on atropine points 212
Leather-felt splints, Mr. Hides's 175
Lepra, Dr. Headland's treatment of 361
Lee, Mr. H., his new instrument for lithotomy 197
—— on syphilitic inoculation in 1865 221
—— on the treatment of syphilis 228
Leg-suspender and bed-guard, Mr Greenway's 172
Leucorrhœa, Dr. Murray on iodine in the treatment of 300
Liebig's food for infants and invalids 326
Ligature of the femoral artery, Prof. Syme on 189
Lithotomy, Mr. Lee's new instrument for 197
Lithotrity, Mr. Henry Thompson on 195
Liver, Sir R. Martin's treatment of diseases of, in India 125
Local anæsthesia, Dr. Bigelow on the use of rhigolene for producing 384
—— anæsthesia, Dr. Richardson's ready method of producing 369
—— pains of the walls of the chest, Sir J. Y. Simpson on chlorocarbon for xxvi

Macgregor, Mr. D., on acute rheumatism, and acupressure 42
Main, Dr. W., on a new uterine support 275
Malignant and recurrent tumours of the orbit, treatment of 213
Mammary tumours with cysts, Mr. Coates on injection of iodine in 315
Martin, Sir R., on hepatic disease amongst Europeans in India 125
Maynard, Mr. J. C., on hyposulphite of soda in diphtheria 119
Meat, Dr. Hassall on the concentration and preservation of 339
—— Dr. Hassall's flour of 338
Medicines, Dr. Brown-Sequard on the administration of 364
Membrana tympani, Dr. Weber on perforation of the 217
Milk, Dr. Scoffern on prevention of sourness in 341
"Missed labour," Mr. Watkins on a remarkable case of 249
Morphia, Dr. Ogle on the subcutaneous injection of in neuralgia 85
——— and atropine, Dr. Brown-Sequard on the subcutaneous injection of 363
Mortality of childbed, Dr. Matthews Duncan on the 323
Munroe, Dr. H., on the action of alcohol on digestion 365
Murray, Dr., on iodine in the treatment leucorrhœa 300
Mustard paper, Mr. Cooper's sinapine tissue or 363

Nævi, Mr. Douglas on the use of the actual cautery for 200
Narcotics, subcutaneous injection of in neuralgia 85
Nephritis, interstitial, Dr. Bence Jones on 134
——— tubular, Dr. Bence Jones on 137
Nervous affections, functional, Dr. Brown-Sequard on 67
——— affections, functional, on the moral treatment of 72
——— affections, functional, on elimination of certain poisons in 315
——— affections, functional, use of bromide of ammonium in sleeplessness of 73
Neuralgia, Dr. Ogle on the subcutaneous injection of narcotics for 85
——— Dr. Richardson on its relief by ether-spray 376
Nitro-muriatic acid bath in hepatic disease, directions for 125

PAGE.
Obstruction of bowels successfully treated by stomach-pump, Dr. Fleming on .. 357
Ogle, Dr J. W., his treatment of chorea by the Calabar bean 83
——— on the subcutaneous injection of narcotics for neuralgia .. 85
Orbit, treatment of malignant and recurrent tumours of the.. 213
Orchitis, acute, Dr Watson on the treatment of 198
Ophthalmia, granular, Dr. Stokes on its treatment by pressure 204
——— gonorrhœal, Mr. Hart on the treatment of 208
——— purulent, Mr. Hart on the treatment of 208
Otoscope, or speculum auris, Dr. Brunton's new 214
Ovarian disease, Mr. Craig on chlorate of potass in.. 296
Ovariotomy, Dr. Aveling's case of 294
——— Dr. Aveling's new coil-clamp for 295
——— Mr. Baker Brown on the actual cautery in 300
Oxygen, Dr. Goolden on the treatment of disease by 49

Paralytic strabismus treated without operation 211
Pelvic articulations, M. Trousseau on loosening of the 320
Perforation of the membrana tympani, Dr Weber on 217
Perineum, Mr Lane's operation for suture of the 290
Perry, Dr. R., his observations of the present epidemic of typhus 27
Pessaries, uterine, Dr. Main's 275
——— uterine, Dr. Wright's 285
Phagedenic ulceration, on inhalation of oxygen in a case of 50
Phthisis, Dr. Wolfe on the use of carbolic acid in 108
Poisoning by prussic acid, Messrs. T. and H. Smith's antidote for 365
Polli, Dr. G., on the sulphites and hyposulphites in zymotic diseases 332
Polydipsia, Dr. Reith's treatment by large doses of valerian 47
Polypus uteri, Dr. Swayne's cases of 270
Popliteal aneurism, Mr. Hilton on the treatment of 191
Porter, Mr. W. E, his method of reducing dislocations of the shoulder-joint . 164
Pressure, instrument for applying, in granular ophthalmia 206
Procidentia uteri, Dr. Barnes on 258
——— uteri, Dr. Marion Sims on the operation for 255
——— uteri, Mr. Chambers on 257
Progressive locomotor ataxy, Dr. Althaus on 51
——— locomotor ataxy, Dr. Jackson's case of 59
——— locomotor ataxy, Dr. Johnson's case of 58
Prolapsus uteri, Dr. Routh on the radical cure by caustic applications.. .. 254
——— uteri, Mr. Ellis on the radical cure of 260
Pruritus senilis, Dr. Beigel on the treatment of 202
Psoriasis, Dr. Headland's treatment of 361
Puerperal fever, Dr. Barnes on 245
——— fever, Dr. Graily Hewitt on 243
——— fever, Dr. Snow Beck on 241
Pulse and temperature, correlation of 21
—— indicator, M. Marey's sphygmograph, or 359
Purdon, Dr. H. S., on the nature and treatment of chilblains 201
Pyæmia and septicæmia, use of the sulphites and hyposulphites in 334
Pyrexial diseases, Dr. Gibson on the use of the thermometer in the diagnosis of 20

Radius, fracture of at the styloid process, use of Gordon's splint for 165
Randolph, Mr. H. W., on typhus fever 35
Ranula, Mr. Coates's case treated by injection of iodine 316
Rectocele, vaginal, Mr. Lane on 287
Reith, Dr., his case of polydipsia treated by valerian 47
Resuscitation after drowning by Drs. Silvester and Marshall Hall's methods combined 342
Rheumatism, acute, Mr. Macgregor on its treatment by blistering 42
——— acute, Mr. Moore on its treatment by blistering 43
Rhigolene, a petroleum naphtha for producing local anæsthesia, Dr. Bigelow on 384

PAGE.
Richardson, Dr. B. W., on a new and ready mode of producing local anæsthesia 369
Roseburgh, Dr., his mode of examining the fundus oculi 204
Routh, Dr. C. H. F., on the radical cure of prolapsus uteri, by caustic applications 264

Sasse, Dr. A., on the physiological operation of the preparations of iron 361
Scoffern, Dr. J., on the preservation of milk 341
Shoulder, Sir W. Fergusson on recurring fibroid disease of the 177
——— joint, Mr. Porter's method of reducing ordinary dislocations of 164
Sick rooms, Dr. Barker on the disinfection of xxxix
Sims, Dr. J. M., on procidentia uteri 255
Sinapine tissue, or mustard paper, Mr. Cooper's 363
Sleeplessness of functional nervous diseases, use of bromide of ammonium in 73
Smith, Mr. G. S., thermometrical observations on the fevers of children 11
Smyly, Dr. P. H., on forcible flexion of the knee for anchylosis in straight position 162
Spasm, Dr. C. H. Jones on 61
Spectacles, blue, preference of at Berlin 212
Speculum auris, Dr. Brunton's new otoscope or 215
——— for examining the larynx, Dr. Labordette's 100
Sphygmograph, M. Marey's, Editor of Lancet on 359
Spina bifida, Mr. Coates's case treated by injection of iodide of potassium 317
Splints, gutta-percha, Mr. Truman's method of making 176
——— leather-felt, Mr. Hides's 175
Sterility, Dr. Barnes on the causes of 303
Stokes, Dr. W. jun., on the treatment of granular ophthalmia 204
Stomach disorders, use of nitric and muriatic acids in certain forms of 128
Strabismus, Mr. Holthouse on its treatment without operation 210
Sulphites and hyposulphites, Dr. Polli on the uses of in zymotic diseases 332
Swayne, Dr. J. G., his cases of polypus uteri, with remarks 270
Syme, Prof., on ligature of the femoral artery 189
Syphilis, Dr. Lee on its treatment by the calomel vapour-bath 234
Syphilitic inoculation in 1865, Mr. Lee on 221
Syphilization, Mr. Lee on 229
——— Mr. Lane on 237
Synovitis, subacute, and contracted joints, Mr. Adams on 163
System, Dr. Bence Jones on the action of alcohol in the 368

Tabes dorsalis, or progressive locomotor ataxy, Dr. Althaus on 51
Tænia, Dr. Headland's treatment by turpentine and castor oil combined 361
Tait, Mr. L., on the treatment of fracture of radius by Gordon's splint 165
Temperature, fluctuations of within the limits of health 20
——— of the body in disease 4
——— range of in disease 22
Thermometer, Dr. Gibson on its use in diagnosis of pyrexial diseases 20
——— a means of diagnosis between tubercular and non-tubercular diseases 100
——— in specific fever, Editor of Lancet on the 7
Thermometrical observations on the fevers of children, Mr. Smith on 11
Thompson, Mr. H., on lithotrity 195
Tracheotomy in croup and diphtheria, Dr. Buchanan on 120
Trousseau, M., on loosening of the pelvic articulations 320
Truman, Mr. E., how to make gutta-percha splints 176
Tubbs, Mr. W. J., his case of hemorrhage from detached placenta 253
Tubercular and non-tubercular diseases, thermometer as a means of diagnosis 100
Tuberculosis, Dr. Dobell on the nature, cause, and treatment of 86
——— on a certain form of hemoptysis not associated with 105
Tubular nephritis, Dr. Bence Jones on 137
Tumour, mammary, Mr. Coates on injection of iodine in 315
Tumours of the orbit, treatment of malignant and recurrent 213
Turning, Dr. Heyderdahl on the use of wire ribbon in difficult cases 246
Tympanic membrane, Dr. Weber on perforation of the 217
Tympanum, Dr. Bishop's instrument for applying remedial agents to cavity of 216

PAGE.

Typhoid fever, Dr. Black on an outbreak of 37
——— fever, Dr. Chambers on the treatment of 40
Typhus fever, Dr. Anderson on the daily excretion of urea in 32
——— fever, Dr. Perry's observations on the present epidemic of .. 27
——— fever, Mr. Randolph on 35
——— fever, daily record of the temperature, &c., in cases of 13

Urea, researches on the daily excretion of in typhus fever 32
Urine, M. Cunisset on chloroform for detecting bile in 365
——— alkaline, Dr. Bence Jones on 131
——— ammoniacal, Dr. Bence Jones on 132
Uterine dilator, Mr. Hembrough's 286
——— leucorrhœa, Dr. Murray on the use of iodine in 300
——— pessaries, Dr. Main's 275
——— support, Dr. Wright's 285
——— polypi, Dr. Swayne on the operation for 273
Uterus, Dr. Wright on extreme flexions of the 279
——— enlargement of, following abortions and confinements, Dr. Beck on .. 251

Vaginal rectocele, Mr. Lane on 287
Valerian, its use in the treatment of diabetes insipidus 48

Walton, Mr. H., on the extraction of black cataract 209
Water, Mr. Spencer on the purification of xxxix
Watkins, Mr. R. W., his remarkable case of "missed labour 249
Watson, Dr. W. S., on the treatment of acute orchitis 198
Weber, Dr. F. E , on difficulty of hearing in perforation of the membrana tympani 217
Wheat phosphates, Dr. Fox on 130
Wolfe, Dr. J. R , on the use of phenic (carbolic) acid in phthisis 108
Wounded ulnar artery, sloughing and secondary hemorrhage, Mr. Hilton on .. 194
Wounds, surgical, Mr. De Morgan on the use of solution of chloride of zinc in .. 158
——— Mr. Paget's cases treated with solution of chloride of zinc 160
Wright, Dr. H. G., on extreme flexions of the uterus 279

Xylo-styptic ether spray, Dr. Richardson on 382

Zymotic diseases, Dr. Polli on the sulphites and hyposulphites in 332

GENERAL INDEX

TO

BRAITHWAITE'S

RETROSPECT OF MEDICINE,

1863—1865.

VOLS. XLVII. TO LII. INCLUSIVE.

	VOL.	PAGE.
Abdominal swellings, Dr. Kennedy on the diagnosis of	50	61
Abortion, imperfect, with retention of ovum for seven months ..	47	338
—— in the early months of pregnancy, Dr. Bell on	52	329
Abscess, hepatic, Dr. Cameron on the treatment of	48	69
Acid and sugar in spirituous liquors, Dr. Bence Jones on	49	392
—— gravel, Dr. Bence Jones on	51	127
—— means of detecting	51	129
—— symptoms produced by	51	130
—— treatment of	51	132
Acidity, consequences and treatment of excess of	51	121
—— on its removal from the body	51	116
—— on the means of detecting in urine	51	117
—— or alkalinity of the urine, Mr. Donovan on estimating the ..	49	82
—— or the acid diathesis, Dr. Bence Jones on	51	113
—— symptoms of excess of	51	119
Acne, Mr. Milton on the treatment of	52	245
—— rosacea, Mr. Ker on the use of armoracia in..	50	194
—— rosacea, treatment of	48	142
Aconite, Dr. Garrod on its external use in neuralgia	49	67
Aconitine, on the preparation of	48	xl
Actual cautery, M. Nelaton's substitute for the	48	358
Acupressure, Prof. Pirrie on	52	159
—— Professor Simpson on	49	105
—— Professor Simpson on its application in ovariotomy ..	49	304
—— Mr. Dix on the substitution of wire for the needles in ..	50	119
—— after amputation, Dr. Keith on cases of	52	162
—— as an hæmostatic agent, Dr. Watson on	52	171
—— for controlling hemorrhage, Prof. Macnamara on ..	52	180
—— its employment in cases of amputation of the thigh ..	49	112
Acupuncture, its early employment in anasarca	47	40
—— its use in the cure of hydrocele	51	250
Acute pneumonia, Dr. Bennett on	47	107
—— rheumatism, Dr. Basham on nitrate of potash in	47	18
—— rheumatism, Dr. Davies on its treatment by blisters	50	xi
—— rheumatism, Dr. Davies on the blister treatment of ..	52	39
—— rheumatism, Dr. Fuller on the treatment of	47	24
—— rheumatism, Dr. Inman on the treatment of	48	12

	VOL.	PAGE.
Adenoceles, or innocent tumours of the breast	50	375
Ague, Mr. Hunter on the hypodermic treatment of	52	90
Albumen in urine, Dr. Clark on best mode of detecting small quantities of	50	169
Albuminuria, Mr. Wearne on the efficacy of arsenic in	47	131
——— in children, Dr. Dickinson on the treatment of	50	385
Alcoholic stimulants in the prevention and treatment of convulsion	48	29
Alkalies, Dr. M'Call Anderson on their use in psoriasis	52	240
Alkaloids of opium, M. Claude Bernard on the	51	xxxix
Alkaline and earthy sulphites in the treatment of catalytic diseases	50	23
——— lotions, employment of in ezema	48	151
——— permanganates as antidotes to organic poisons	47	371
——— sulphites in the treatment of zymotic diseases	52	1
Alopecia, Mr. Milton on the treatment of	52	221
——— areata, treatment of	49	173
——— areata, Dr. Watson's case treated by carbolic acid	50	188
——— or falling off of the hair, Dr. Frodsham on	50	186
Alum injections in the treatment of gonorrhœa	47	225
Amaurosis, Mr. Hart on tobacco-smoking as a cause of	48	214
——— cerebral, Mr. Hutchinson's clinical data respecting	50	274
——— intra-cranial, Mr. Hutchinson on	51	299
——— produced by tobacco, Mr. Wordsworth on	48	212
Amenorrhœa, Dr. Corlieu on the use of apiol in	50	371
Ammonæmia, Dr. Harley on	49	398
Amputation, Prof. Fergusson on	52	138
——— Dr. Keith's cases of acupressure after	52	162
——— Mr. Porter on a new mode of arresting venous hemorrhage after	49	124
——— at the hip joint, Mr. Spence's case of	47	144
——— at the thigh, Prof. Fergusson on	52	145
——— by single flap, Mr. Carden on	52	134
——— by single flap, Mr. Carden on	49	xxii
——— of thigh, by Teale's operation, Mr. Porter on	49	99
A model hysteric	47	347
Anæmic sphacelus of the fingers and toes	48	159
Anæsthetic, Dr. Arnott's mode of using congelation as an	48	372
Anal fissure, Mr Lane on polypus of the rectum with	52	200
Anasarca, Dr. Nevins on the treatment of	47	40
Anchylosis, Mr. Adams on restoration of motion in cases of partial	51	186
Aneurismal clamps, Mr. Hart on the application of indices to	49	127
Aneurism, Dr. Mapother's case of osteo-aneurism	47	177
——— Dr. O'Ferrall on combined distal and proximal compression in	51	192
——— Dr. Roberts on the use of iodide of potassium in	47	171
——— of ascending aorta, Mr. Moore's case treated by insertion of wire	49	129
——— of the popliteal, Dr. Porter's case of	52	183
Aniline and nitrobenzole, physiological properties of	48	388
——— sulphate of, its efficacy in hysterical chorea	51	53
Ankle-joint, Mr. Hancock on excision of the	47	150
Anthrax, Mr. Collis on its treatment by pressure	49	181
Antilactescent, Dr. Morris on iodide of potassium as an	50	329
Antiseptic, Dr. Watson on carbolic acid as an	49	375
Antispasmodics in typhoid fever, Dr. Osborn on	50	29
Aphonia, Dr. Mackenzie on its treatment by galvanism	48	49
Apiol, Dr. Corlieu on its use in amenorrhœa and dysmenorrhœa	50	370
Areca nut, its use in the treatment of tapeworm	47	116
Armoracia (root of horseradish), its use in acne rosacea	50	194
Arsenic, Dr. M'Call Anderson on its use in psoriasis	52	238
——— Mr. Milton on its use in acne	52	245
——— Mr. Milton on its use in eczema	52	229
——— its efficacy in albuminuria	47	131
——— its employment in eczema	48	149
Arsenical preparations, on the treatment of neuralgia by	49	67
Arteries, Mr. Churchill on a new mode of tying	52	185
Arthritic posterior internal ophthalmia, or acute glaucoma	50	267
Articular cartilages, Mr. Holmes on exposure of in wounds	51	176
——— extremities of bones, Mr. Bryant on inflammation of the	48	93

	VOL.	PAGE.
Artificial pupil, and extraction of cataract, Mr. Solomon's case of ..	51	296
——— respiration, its application in suspended animation ..	47	360
——— teeth, Dr. Wells on the ill effects of red vulcanite used for ..	48	357
Ascites, Dr. Nevins on the treatment of	47	41
——— and ovarian dropsy, Mr. Spencer Wells on the diagnosis between	50	362
Assimilation, defective, Dr. Pavy on its influence on the urine ..	48	323
——— defective, its influence on the urine	47	124
Asthma, Dr. M'Veagh on the use of datura tatula in	48	59
——— Dr. Salter on the value of chloroform in	51	82
——— Dr. Salter on its treatment by iodide of potassium	49	71
——— Dr. Sanderson on	48	57
Ataxy, progressive locomotor, Dr. Radcliffe on	52	53
Atomized fluids, Dr. Beigel on the inhalation of	52	102
Atropia, Dr. Fleming on the action and uses of	48	363
——— Dr. Fleming on the internal exhibition of	47	374
Atrophy, muscular, Dr. Radcliffe on	51	44
Atropine paper, Mr. Streatfeild on the use of	48	211
Atropized and Calabarized gelatine for dilating or contracting the iris..	49	202
Auscultatory signs which precede crepitation in pneumonia ..	51	73
Auto-laryngoscopy, Dr. Johnson's simple method of	47	111
Azoturia, Dr. Sieveking on	52	121

Backache, Dr. Gervis on a variety of	49	347
Belladonna, Dr. Hardy on its use in distension of the breasts ..	50	369
——— Dr. Morris on its effects in opium poisoning	47	382
——— Dr. Thorowgood on its use in epilepsy	50	36
——— M. Courty on hypodermic injections of in opium poisoning	47	383
——— Mr. Hogg on its value in iritis	50	235
——— Mr. Hutchinson on its use in cases of severe burn ..	49	180
——— and sulphate of zinc in hooping-cough	48	318
——— its use in the treatment of epilepsy	47	43
——— its injurious action in glaucoma	50	265
——— poisoning, Dr. Fraser on the treatment of	50	xxxvi
Benzine, Dr. Mosler on its efficacy in trichiniasis	51	277
Biniodide of mercury in gonorrhœal rheumatism, Dr. Fuller on ..	47	35
Bismuthi, liquor, Mr. Schacht's improved preparation of	48	342
Bladder, Prof. Fergusson's case of extraction of a bougie from the ..	48	126
——— Dr. Humphry's case of bone, teeth, hair, &c., extracted from the	50	175
——— Mr. Porter on the extraction of a hair-pin from the ..	49	333
——— Mr. Smith's case of a portion of tobacco-pipe broken into ..	49	167
——— displacement of, as a cause of tedious labour	49	349
——— irritable, efficacy of triticum repens in	47	132
Bleeding in injuries of the head, Dr. Ashhurst on	50	129
Blister treatment of acute rheumatism	52	39
Bloodletting in certain cases of convulsion	48	29
——— in the treatment of puerperal peritonitis	48	286
Borax as a substitute of ergot, Mr. Moore on	50	323
Bowel, intussusception of, Dr. Greig on inflation as a remedy in ..	50	68
Bougie, Prof. Fergusson's case of extraction of, from the bladder ..	48	126
Bougies, M. Nelaton on vulcanized caoutchouc for	48	126
Breast, Dr. Hardy on the use of cere cloths in painful affections of the	50	369
——— Mr. Bryant on the diagnosis of tumours of the	50	374
——— Mr. Bryant on excision of the	50	382
Bright's disease, Dr. Hassall on the treatment of	51	148
——— Dr. Jenner on	51	142
Bromide of ammonium, Dr. Harley on its use in hooping-cough ..	49	76
——— of ammonium, Dr. Ritchie on its use in hooping-cough ..	50	57
——— of ammonium, its use in hooping-cough	48	321
——— of ammonium, its use in irritable uterus	49	330

	VOL.	PAGE.
Bromide of potassium, Dr. Behrend on its action in inducing sleep	50	xxxv
——— of potassium, Dr. Garrod on	49	378
——— of potassium, Dr. Rogers on its use in epilepsy	51	47
Bromides in the water of the Dead Sea	48	391
Bromine, solution of, Dr. Smith's formula for	48	355
Bronchial tubes, Dr. Gairdner on the deobstruent function of the	48	64
Bronchitis, acute, sedative action of the Calabar bean in	48	200
——— acute, use of tannin in	48	52
——— and fever as early signs of phthisis	51	68
——— chronic, Dr. Greenhow on tincture of larch in	49	75
——— chronic, use of the verbascum thapsus in	47	xviii
——— rheumatic, Dr. Jones's treatment of	51	80
Broncho-pneumonia of lying-in women, Dr. Barnes on the	48	281
Bullet, the travels of a	50	131
Bullets in wounds, M. Nelaton's instrument for discovering	47	169
——— M. Favre's galvanic probe for discovering	47	170
Burns, Mr. Hutchinson on internal administration of belladonna in severe	49	181
——— Mr. Skey on the treatment of	49	178
——— on the treatment of	47	224
Bursæ, enlarged, Mr. Skey on the treatment of	49	187
——— mucosæ, Prof. Inzani on the importance of tapping	48	385

Calabar bean, Dr. Fraser on the preparations and therapeutical uses of	48	199
——— Dr. Ogle on some effects of its application to the eye	48	206
——— Dr. Ogle on its effect in obstinate chorea	52	71
——— Dr. Robertson on its uses in ophthalmic diseases	48	202
——— Dr. Wells on its effects on the accommodation of the eye	48	195
——— Mr. Hart's case of mydriasis treated by the	48	204
——— Mr. Nunneley on its employment in prolapsus of the iris	48	203
——— Mr. Nunneley on the action, preparations, and use of the	49	205
——— paper, Mr. Hart on the effect of	49	209
Calculi, Dr. Roberts on their solution in the kidney and bladder	51	249
——— passage of from the kidneys or bladder, treatment of	51	133
——— phosphatic, Dr. Harley on	51	136
——— uric acid, Dr. Harley on the treatment of	50	83
Calomel vapour bath, cases of syphilis treated by	50	285
Cancer, Mr. Hunter on the hypodermic treatment of	52	90
——— epithelial, Mr. Bryant on	48	23
Cancerous stricture of the rectum, Mr. Curling's case of	51	223
——— tumours of the breast	50	376
Carbolic acid, Dr. Calvert on the therapeutic properties of	48	367
——— Dr. Watson on	49	374
——— Dr. Watson on its use in alopecia areata	50	188
——— Dr. Watson its use in parasitic skin diseases	51	xxviii
——— Dr. Watson on its use for carious teeth	51	xvii
Carbuncle, Dr. Pattison on the use of carbolic acid in	48	367
——— Mr. French's successful treatment by subcutaneous section	52	259
——— Mr. Prichard on the caustic treatment of	48	154
——— and sloughing sores, use of carbolic acid in	49	374
——— on its treatment by pressure	49	181
Cardiac affections, scarlet fever complicated with	47	1
——— disease, Dr. Sutton on digitalis in	49	68
——— disease, Dr. Murray on digitalis in	51	xiv
Caries of single bones of the tarsus and metatarsus, Mr. Holmes on excision in	51	181
Carious teeth, Dr. Watson on the use of carbolic acid for	51	xvii
Castor oil, new mode of administering	47	xxxviii
Cataract, Dr. Graefe on the operation for	52	277
——— Dr. Mooren's method of extraction of	51	286
——— Dr. Taylor on the improved methods of extraction of	51	284

	VOL.	PAGE.
Cataract, Mr. Bowman on extraction of by a traction-instrument	52	291
——— Mr. Critchett on its removal by the scoop method	52	280
——— Mr. Critchett's Vectis spoon for the extraction of	50	244
——— Mr. Hart on the extraction of	47	235
——— Mr. Solomon's case of extraction of, and formation of artificial pupil	51	296
——— Mr. Teale on the extraction of by suction	50	238
——— Jacobson's method of extraction of	51	292
——— Schufte's method of extraction of	51	294
——— congenital, Mr. Critchett's treatment of by iridesis	50	246
——— form of spoon employed by Mr. Critchett for	52	283
——— inutility of the candle-test in	47	236
Catarrhal inflammation of the auditory meatus and tympanum	49	250
——— ophthalmia, Dr. Henderson on	48	191
——— ophthalmia, Mr. Hancock on the treatment of	52	265
Catheterism in cases of hypertrophy of the prostate	48	124
Catheters, use of vulcanized caoutchouc for	48	126
Catoptric state of the lens, how to test the	47	237
Cauliflower excrescence of the uterus, Drs. Routh and Hicks on the ecraseur in	51	366
Caustic, Mr. Prichard on the treatment of carbuncle by	48	154
——— holder, Mr. Ellis's new instrument for use in uterine disease	48	315
——— new mode of preparing	47	xxxviii
Caustics, Dr. Morton on certain abuses of	49	368
Cauterisation, M. Nelaton's new method of	48	358
Cellulitis, pelvic, in a case of ruptured vagina	48	225
Cerebral amaurosis, Mr. Hutchinson's clinical data respecting	50	274
——— diseases, Mr. Hunter on the hypodermic treatment of	52	85
Cere cloths, use of in painful affections of the female breast	50	369
Cervix uteri, Dr. Barnes' case of hypertrophic elongation of	47	344
——— Dr. Barnes on division of in dysmenorrhœa	52	361
——— Dr. Gream on dilatation versus division of, in dysmenorrhœa	51	391
——— Dr. Kidd on division of in dysmenorrhœa	52	356
——— Dr. Priestley on an instrument for dilating	49	306
——— Mr. Ellis on an instrument for dilating	49	309
Chancre and syphilitic lichen with intense pruritus, Dr. Sisson on	51	325
Child-crowing, Mr. J. Roberton on exposure to dry cold winds in	51	397
Children, Dr. Dickinson on the treatment of albuminuria in	50	385
——— M. Guersant on polypus of the rectum in	50	383
Chlorate of potash, its use in syphilitic sore throat	47	226
Chlorine mixture for scarlet fever, formula for	47	xiv
Chlorodyne, Mr. Ashburner's formula for preparing	50	xxxvi
Chloroform, Dr. Braithwaite on its use in the treatment of cholera	52	400
——— Dr. Clay on its use in ovariotomy	49	288
——— Dr. Cuthill on its use in puerperal convulsions	52	348
——— Dr. Kidd on its use in puerperal convulsions	49	354
——— Dr. Parson on its administration in ovariotomy	51	374
——— Dr. Salter on its use in asthma	51	82
——— and its safe administration, Dr. Kidd on	49	379
——— its local application in neuralgia and muscular rheumatism	50	42
——— on the administration of	50	xxxvi
——— syrup of, Mr. Groves's formula for	50	xxxvi
Chloroformic anodyne, Dr. Harley's case of poisoning by	48	343
Cholera, Dr. Bell on its treatment by bleeding	52	398
——— Dr. Bell on the pathology of	52	395
——— Dr. Braithwaite on its pathology and treatment	52	385
——— Dr. Bullar's case treated by the hot mustard bath	52	406
——— Dr. Hill on the treatment of	52	400
——— Dr. Johnson on the pathology of	52	386
——— Dr. Massy on the symptoms and treatment of	52	393
——— Dr. Maxwell on its effects on the system	52	393
——— Dr. Ricketts on the subcutaneous injection of morphia in	47	117
——— Dr. Wallis on the cause of cholera and its effects on the system	52	391
——— Dr. Wise on the hemostatic treatment of	48	19
——— Mr. Brady's case treated by chloroform	52	401
——— Mr. Plimmer's case treated by chloroform	52	402

	VOL.	PAGE.
Cholera, Mr. Steadman's case treated by chloroform	52	402
—— and its prevention, Dr. Mapother on	50	15
—— consecutive fever of	52	407
—— its treatment by calomel	52	407
—— its treatment by chloroform	52	400
—— its treatment by cold water	52	407
Chopart's operation, and excision of the ankle-joint	47	146
Chorea, Dr. Kirkes on the treatment of	48	31
—— Dr. Turnbull on the general treatment of	51	56
—— hysterical, its successful treatment by sulphate of aniline	51	53
—— obstinate, Dr. Ogle on the effect of Calabar bean in	52	71
—— treated by alcoholic stimulants	48	29
Chronic ulcers, Mr. Skey on the treatment of	49	189
Ciliary blepharitis, Dr. Fano on the treatment of	49	204
—— muscle, Mr. Hancock on division of the	49	240
Cinchonine, its substitution for quinine, to detect	48	343
Citrate of ammonia in cases of irritable bladder	48	81
Clay as a dressing for sores	48	159
Cleft palate, Prof. Fergusson on the operation for	50	135
—— Mr. Collis on	51	213
—— congenital, Dr. Kingsley on the treatment of	51	205
Climate, Dr. Bennet on the influence of sudden change of	48	374
—— of Egypt, Dr. Adams on the	48	332
Codeia, Dr. Garrod on	49	363
Cod-liver oil, Dr. M'Call Anderson on its use in psoriasis	52	239
—— Mr. Milton on its efficacy in eczema	52	228
—— and iron, their efficacy in eczema	48	146
Collodion with glycerine in erysipelas, preparation of	48	357
Colon, Dr. Kennedy on dilatation of the	50	63
Compress, wire, as a substitute for the ligature	50	113
Compression in certain cases of aneurism, Dr. O'Ferrall on	51	192
Condylomata, Dr. Finco on mercurialised collodion as an application to	51	326
Congelation as an anæsthetic. Dr. Arnott's improved method of using	48	372
Congenital rupture at the groin, on a simple support for	47	185
Conjunctiva, Dr. Sheraton on tannin in inflammatory affections of the	48	186
Conjunctivitis, Dr. Pagenstecher on the treatment of	52	267
—— granular, its treatment by inoculation with pus	50	220
Consumption, Dr. Cotton on the therapeutics of	50	50
—— Dr. Mapother on the prevention of	50	16
—— influence of living in the open air in	50	56
Contagion, Dr. Lionel Beale on	52	9
Convulsion, Dr. Radcliffe on the therapeutics of	48	25
Convulsions, puerperal, Dr. Cuthill on the use of chloroform in	52	348
—— puerperal, Dr. Kidd on the use of chloroform in	49	354
—— puerperal, Mr. Jones on the treatment of	48	234
—— puerperal, advantages of chloroform over bleeding in	48	236
—— puerperal, cases treated with and without bleeding	48	241
Cornea, Mr. Carter on iridectomy in destructive ulceration of the	48	192
—— Mr. Jordan on eczema of the	51	270
—— Mr. Watson on the use of the seton in vascular ulcers of the	51	281
—— vascular, Dr. Bader on syndectomy in	48	178
—— vascular, Mr. Hutchinson's treatment by purulent inoculation	52	275
—— vascular, with granular lids, Mr. Lawson's case of	48	175
Corneitis phlyctenulosa, Dr. Pagenstecher on the treatment of	52	267
Corrosive sublimate poisoning, Dr. Johnson on gold dust and iron filings in	48	347
Craniotomy, Dr. Inglis on the advantages of turning preliminary to	51	337
Creasote, its action in the treatment of sycosis	51	273
Croton oil in neuralgia and rheumatism, Dr. Sewell on	52	67
Croup, Dr. Hamilton on the treatment of	48	317
Cyanide of potassium, Dr. Davanne on poisoning by	48	353
Cysts, development of in tumours of the breast	50	377
Cystic tumour of the vagina, Dr. Barnes' case of	47	261

	VOL.	PAGE.
Datura tatula, its use in the treatment of asthma	48	59
Dead, apparently, Royal Humane Society's instructions for restoring the	48	340
—— body, Dr. Richardson's attempt to restore the colour of a putrid	48	336
—— sea, M. Roux on the bromides of the	48	391
Death from chloroform, Dr. Kidd on the cause of	49	382
Deformed pelvis, induction of premature labour in a case of	52	327
Deformity, Mr. Paget on a remarkable case of	52	337
Delirium tremens, Dr. Duchesne on digitalis in	47	65
—— —— Dr. Miller on its treatment by digitalis	48	34
—— —— Dr Perrie on its treatment	47	55
—— —— Dr. Seller on	49	33
—— —— Dr. Sloane on its treatment by digitalis	49	45
—— —— Mr. Hunter on the hypodermic treatment of	52	86
—— —— Mr. Kesteven on digitalis in	47	64
—— —— eliminative and supporting plan of treatment of	47	62
—— —— sedative action of the Calabar bean in	48	200
Delivery, Mr. Rigden's case of inversion of the uterus after	51	344
—— by spontaneous evolution, Dr. Hodges's case of	48	255
Depletion and excessive (?) stimulation, Dr. Beale on	48	16
Deposits of urates, Dr. Harley on the pathology of	50	80
Diabetes, Dr. Harley on the pathology of	52	124
—— Dr. Bence Jones on the treatment of	51	152
—— Dr. Noble on pathological distinctions in cases of	47	119
—— Dr. Smith on the proper diet in	49	92
—— Dr. Venables on	48	75
Diabetic sugar, MM. Trousseau and Dumontpalier's new test for	48	80
Dialysis, Dr. Harvey on detection of poisons in medico-legal cases by	47	366
Diet of children, Mr. Erasmus Wilson on the	51	xl
—— —— Baron Liebig's soup	51	xl
—— of lying-in women, Dr. Graily Hewitt on	50	324
Digital pressure, aneurism of the popliteal cured by	52	183
Digitalis, Dr. Dickinson on the action of	47	xxxviii
—— Dr. Fuller on the action of	47	105
—— Dr. Gull on its action on enfeebled hearts	52	91
—— Dr. Henry on its action as a cardiac tonic	52	99
—— Dr. Murray on its action in heart diseases	52	93
—— Dr. Robertson on its use in insanity	49	46
—— Dr. Sloane on its use in delirium tremens	49	45
—— Dr. Sutton on its employment in cardiac disease	49	68
—— in cardiac affections, Dr. Leared on the action of	47	105
—— in delirium tremens, Dr. Duchesne on	47	65
—— in delirium tremens, Dr. Miller on	48	34
—— in delirium tremens, Mr. Kesteven on	47	64
—— its value as a cardiac tonic	47	101
Dilatation of the cervix uteri, Dr. Priestley on an instrument for	49	306
—— of the colon, Dr. Kennedy on	50	63
—— versus division of the cervix uteri in dysmenorrhœa, Dr, Gream on	51	391
Diphtheria, Dr. Gibson on the treatment of	49	78
—— Dr. Morton on the abuse of caustics in	49	370
—— Mr. Turner on the use of carbolic acid in	48	368
—— spurious, Mr. G. S. Smith on its nature and treatment	49	80
Disease like measles arising from an unusual cause	47	2
Disinfectant, Dr. Moffatt on phosphorus as a	52	19
—— Dr. Richardson on iodine as a	48	356
—— Dr. Watson on carbolic acid as a	49	375
—— Mr. Druitt on iodine as a	49	377
Dislocation of shoulder, Mr. Salmon on reduction of by slow manœuvres	49	98
—— of the thumb at the metacarpo-phalangeal joint	48	96
Displacement of the bladder as a cause of tedious labour, Dr. Broadbent on	49	349
Dropsy, Dr. Johnson on the pathology, prognosis, and treatment of	49	20
—— Dr. Smith on the use of Erodium cicutarium in certain forms of	50	96
Dysentery, Dr. Henderson on the efficacy of mineral acids in	48	67
Dysmenorrhœa, Dr. Barnes on the treatment of	52	360
—— Dr. Bennet's treatment of	52	365

	VOL.	PAGE.
Dysmenorrhœa, Dr. Corlieu on the use of apiol in	50	373
——— Dr. Gream on dilatation versus division of the cervix-uteri in	51	391
——— Dr. Greenhalgh's treatment of	52	362
——— Dr. Hewitt's treatment of	52	363
——— Dr. Kidd on	52	352
——— Dr. Routh's treatment of	52	362
——— Dr. Sims on excision of the os and cervix uteri in	51	385
——— Mr. Baker Brown's treatment of	52	361
——— Mr. Hunter on the hypodermic treatment of	52	90
Dyspepsia, Dr. Leared on charcoal in	52	107

Ear, Dr. M'Call Anderson on eczema of the external passages of	48	215
Early mortality, Dr. Whitehead on the causes of	47	355
—— pregnancy, Dr. Routh on some symptoms of	51	331
Echinococcus hominis, Dr. Leith Adams's case of	50	93
Ecraseur, Dr. Marion Sims on its use for removal of uterine polypi	51	381
——— Drs. Routh and Hicks on the merits of the wire-rope	51	366
——— its use in the treatment of hemorrhoids	47	190
Eczema, Prof. Hebra on the diagnosis of	47	217
——— Dr. M'Call Anderson on the constitutional treatment of	48	144
——— Dr. M'Call Anderson on the local treatment of	48	xxv
——— Dr. M'Call Anderson on the treatment of	50	196
——— Dr. Wallace on weak alkaline lotions in	48	151
——— Mr. Meade on the treatment of	51	268
——— Mr. Milton on external applications in	52	234
——— Mr. Milton on the treatment of	52	228
——— Mr. Startin's treatment of	52	234
——— Mr. Wilson's treatment of	52	234
——— Mr. Wilson on the nature, varieties, and treatment of	51	259
——— meatus, Dr. M'Call Anderson on	48	215
——— not a vesicular disease	52	226
——— of the eyelids, conjunctiva, and cornea, Mr. F. Jordan on	51	269
——— with gouty complication, treatment of	48	140
Egypt, Dr. Adams's notes referring to invalids proceeding to	48	327
Electricity, its efficacy in an obstinate case of hysteria	47	351
——— its employment in hydrophobia and tetanus	47	53
Electro-magnetism in post-partum hemorrhage, Mr. Parsons on	52	335
Embolia, Mr. Cooper's cases of	47	298
——— M.M. Gouraud and Lancereaux on a case of	47	301
——— of lying-in women, Dr. Barnes on the	48	277
Emprosthotonos, Dr. Edmunds' treatment by ice to the spine	49	61
Endoscope, Dr. Cruise's description and use of the	51	254
——— M. Desormeaux's, its application in diseases of the urinary passages	48	134
Enfeebled heart, Dr. Jones on the value of digitalis in	47	101
Enlarged ovary, Dr. Brisbane on tapping through the vaginal wall for	49	303
——— tonsils, Dr. Mackenzie on their treatment without cutting	50	143
——— tonsils, Dr. Smyly on the treatment of	51	220
Enteric fever, Dr. Christison on	49	5
Entropium, Dr. Snelling's operation for	47	256
——— Mr. Critchett on the treatment of	47	253
Epilepsy, Dr. M'Donnell on bromide of potassium in certain forms of	49	51
——— Dr. Radcliffe on the treatment of	48	25
——— Dr. Ramskill on its treatment by belladonna	47	43
——— Dr. Rogers on the use of bromide of potassium in	51	47
——— Dr. Thorowgood on the use of belladonna in	50	36
——— syphilitic, Dr. Ramskill's case of	48	33
——— with insanity, Dr. Chapman's case of	52	64
Epiphora, Mr. Critchett on	49	191
Epiphyses, Mr. Bryant on inflammation of the	48	93
——— of long bones, Mr. Hutchinson on separation of the	50	131

	VOL	PAGE.
Epistaxis, Dr. Cahill on plugging the nose in	47	181
Epithelioma, or epithelial cancer, Mr. Bryant on	48	23
Epitheliophyta, Bazin's treatment of	49	174
Ergot, Dr. Brown on its use in inefficient action of the uterus	52	324
——— Dr. Gill on its action on the uterus	47	264
——— Dr. Graily Hewitt on its action on the uterus	47	262
——— Dr. Lumley Earle on the quality and mode of administration of	50	311
——— Dr. Simpson on its use in inducing uterine action	52	326
——— Mr. Moore on borax as a substitute for	50	323
——— Mr. Wenzell on the active constituents of	50	322
——— Mr. Wenzell on the active constituents of	52	324
——— in menorrhagia, Dr Graily Hewitt on	47	353
——— premature labour induced by	48	233
Erodium cicutarium (stork's bill) as a remedy in certain forms of dropsy	50	96
Erysipelas, Dr. Smith's preparation of collodion with glycerine in	48	357
——— sedative action of the Calabar bean in	48	200
Erythema nasi—gouty nose	47	15
Eustachian tubes, Dr. Anderson on obstruction of the	49	242
Excision of the ankle, and Chopart's operation	47	146
——— of the knee-joint, Prof. Fergusson on	50	99
——— of the knee-joint, Dr. Barton on	51	174
——— of the knee-joint, Mr. Butcher on	47	137
——— of the knee-joint, Mr. Butcher on	48	86
——— of the knee-joint, Mr. Le Gros Clark on	49	96
——— of the knee-joint in children, Mr. Smith on	48	90
——— of single bones of the tarsus and matatarsus for caries	51	181
——— of the tongue, Prof. Syme on	51	194
Expectorant remedies, Dr. Gairdner on the action of	48	60
Extraction of cataract, Dr. Mooren on	51	234
——— of cataract, Mr. Critchett's Vectis spoon for	50	244
——— of cataract, Mr. Teale's suction-curette for	50	241
Extra-peritoneal operation for strangulated hernia, Mr. Jordan on the	50	153
——— operation in strangulated hernia, simplification of the	50	148
Extreme divergent strabismus, Mr. Solomon on	51	301
Eye, Dr. Wells on the effects of Calabar bean on the accommodation of the	48	195
——— Mr Lawrence on an improved speculum for the	52	276
——— Mr. Vose Solomon on reflex disease of traumatic origin	50	275
——— Mr. Streatfeild's forceps to fix the globe in operations on	48	172
Eyelashes, Flarer's operation for extirpation of the	51	278
——— extirpation of the bulbs of, for entropium	47	257
Eyelids, Mr. F. Jordan on eczema of the	51	269
——— Mr. Vose Solomon on syphilitic tubercle of the	48	333
——— granular, Mr. Lawson's case of	48	175
——— nævus of, treated by puncture with a hot needle	47	179

Facial neuralgia, Mr. Craig on acupuncture in	50	38
Fæcal vomiting in a case of ileus treated by galvanism	51	102
Faradization, Dr. Skinner on the galactagogue properties of	49	279
——— in certain forms of paralysis, Dr. Althaus on	52	49
Fatty degeneration, Dr. Mapother on	50	18
——— disease of the heart, Dr. Kennedy on	50	44
Favre's galvanic probe for discovering metallic bodies in wounds	47	170
Favus, Dr. Watson on the efficacy of carbolic acid in	49	376
——— Mr. Wilson on the treatment of	49	171
Febrifuge properties of the common white willow, M. Cazin on the	50	31
Feet, ill-smelling M. Hebra on the treatment of	52	263
Female, Mr. Lane's case of lithotomy in a	47	210
Femoral hernia, strangulated, Mr. Prescott Hewett on	47	182
Femur, fractured, case of perfect bony union of	47	156

VOL. PAGE.
Fetid uterine discharges from imperfect delivery, Dr. Matthews Duncan on 47 336
Fever, Dr. Christison on the prevalence of in large towns 49 1
——— Dr. Gull on the treatment of 50 12
——— Mr. Hunter on the hypodermic treatment of 52 90
——— Mr. Leach on the use of stimulants in 51 23
——— contagious, Dr. Beale on the nature of the poison of .. 52 10
——— enteric, Dr. Christison on 49 5
——— eruptive, in puerperal patients, Prof. Simpson on 48 270
——— gastric, Dr. Christison on 49 5
——— intermittent. Dr. Ward on 50 18
——— malarious, Mr. Hare on the treatment of 51 24
——— puerperal, Dr. Barnes on 52 343
——— puerperal, Dr. Barnes on the constitutional and local causes of 51 349
——— puerperal, Dr. Snow Beck on 52 339
——— puerperal, Dr. Latham on 47 295
——— puerperal, M. Grisar on its propagation by accoucheurs .. 51 360
——— relapsing, Dr. Christison on 49 1
——— remittent, Dr. Purdon on the hypophosphites in .. 52 6
——— rheumatic, Dr. Davies on the blister treatment of .. 51 36
——— rheumatic, Dr. Dobell on the germs of .. 52 37
——— rheumatic, Dr. Everett on the use of alkaline baths in .. 51 40
——— rheumatic, Dr. Nevins on the treatment of 48 9
——— rheumatic, Mr. Roberton on the treatment of 50 21
——— scarlet, Dr. Addison on 52 20
——— scarlet, Dr. Fenwick on condition of stomach and intestines in 50 9
——— scarlet, Dr. Murchison on the etiology of 50 1
——— scarlet, Mr. Blyth on the chlorine treatment of 49 7
——— scarlet, Mr. Milton on the treatment of 52 27
——— scarlet, Mr. Morris' formula for the chlorine mixture for .. 47 xiv
——— scarlet, influence of age on liability to 50 8
——— scarlet, its complications with rheumatic and cardiac affections 47 1
——— typhoid, Dr. Henderson on hydrochloric acid in 48 68
——— typhoid, in Penicuik, Dr. Thin on 51 13
——— typhus, Dr. Christison on 49 2
——— typhus and typhoid, Dr. Gairdner on the treatment of .. 51 7
——— typhus and typhoid, Dr. Wilkes on the treatment of .. 51 1
——— typhus and typhoid, Ed. of Lancet on the use of stimulants in 51 4
Fibro-cellular tumour, malignant, Dr. Buchanan on 49 186
Fibrinous clotting in the bloodvessels 47 297
Fibrous tumour of the uterus, Dr. Duncan on source of hemorrhage in 49 320
——— of the uterus, Dr. Routh on the diagnosis of .. 50 330
——— of the uterus, Dr. Sadler's case of 49 319
——— of the uterus, Dr Routh on the treatment of .. 50 336
——— of the uterus, Mr. I. B. Brown on 49 316
——— of the uterus, Mr. Brown's operation for .. 51 362
First stage of labour, Dr. Inglis on the facilitation of the .. 52 318
Fistula lachrymalis, Mr. Critchett's treatment of 49 200
——— vesico-vaginal, Dr. Banon on the surgical treatment of .. 51 395
——— vesico-vaginal, Mr. I. B. Brown on 47 282
——— vesico-vaginal, Mr. Pollock on the operation for 49 315
——— vesico-vaginal and recto-vaginal, Mr. Bryant on .. 52 371
——— vesico-uterine, Mr. Lane on 47 283
——— vesico-uterine, Mr. Lane on a case of 49 310
——— vesico-utero-vaginal, Mr. Spencer Wells on 47 291
Fistulæ, Mr. Turner on the use of carbolic acid in 48 368
Flatulence, Dr. Leared on a novel use of charcoal in 52 107
Flooding, Dr. Barnes on perchloride of iron to arrest 51 349
Fœtal head, Dr. Hicks on the best mode of delivering after perforation 51 345
Forceps, Dr. Gayton's, and mode of securing the handles during delivery 48 251
——— Dr. Lawrence on the employment of the 47 271
——— Mr. Streatfeild's, to fix the globe in eye operations 48 172
——— Mr. Thompson on the use of the 49 255
——— cases, Dr. Ricketts on prevention of laceration in .. 47 275
Forcible extension in cases of partial anchylosis, Mr. Adams on .. 51 186

VOL. PAGE.
Fractured femur, Mr. James' case of perfect bony union of 47 156
Fractures, Dr. Lyon on 52 146
——— Mr. Skey on the treatment of 49 95
——— Mr. Tufnell on the immediate treatment of by fixed apparatus 51 163
——— principal points in the treatmeant of by fixed apparatus 51 173
——— ununited, Mr. Bickersteth's new operation for union of .. 49 93
Fundus oculi, Dr. Roseburgh on a simple mode of examining 52 277
Fungus hematodes, Dr. Pattison on the efficacy of carbolic acid in .. 48 369
Funis, prolapse of, Dr. Earle on the treatment of 49 277

Galactagogue properties of Faradisation 49 279
Galium aperinum, Mr. Bulley on use in hard nodulated tumour of the tongue 50 141
Gall-stone colic, Dr. Thudichum on 48 73
Galvanism, its direct application to the vocal cords in aphonia .. 48 49
——— its use in a case of opium poisoning 48 352
Galvanization in certain forms of paralysis, Dr. Althaus on .. 52 52
Ganglions, Mr. Skey on the treatment of 49 188
Gangrene, hospital, Mr. Maunder on 50 209
Gastrodynia, Dr. Leared on oxide of manganese in 51 101
Gastrotomy in a case of rupture of uterus: subsequent pregnancies 52 331
Gelatine atropine paper, Mr. Streatfeild's 49 202
——— atropized and Calabarized, for dilating or contracting the iris, Hart's 49 202
Glands, inflamed, Dr. Prieur on the use of iodine in 51 276
Glaucoma, Mr. Bowman on iridectomy in 49 221
——— Mr. Browne on iridectomy in 49 235
——— Mr. Critchett on iridectomy in 49 211
——— Mr. Hildige on iridectomy in a case of 49 238
——— Mr. Hogg on iridectomy in 49 237
——— Mr. H. Walton on iridectomy in 49 213
——— Mr. Wharton Jones on the pathology of 50 262
——— Mr. Wharton Jones on iridectomy as a means of treating 50 268
Glycerine as a vehicle for applications to the larynx, Dr Johnson on 50 148
Goitres, parenchymatous, M. Luton's use of local injections in .. 50 43
Gold dust and iron filings an antidote to corrosive sublimate .. 48 347
Gonorrhœa, Dr. Henderson on two new specific remedies for .. 51 328
——— Dr. Collis on the use of injections in 47 225
Gonorrhœal ophthalmia, Dr. Collis on 47 257
——— orchitis, Mr. Smith's treatment of 50 182
——— rheumatism, use of biniodide of mercury in 47 35
——— rheumatism, Dr. Popham on tincture of larch in .. 49 16
Gout, Dr. Bence Jones on the treatment of 52 29
—— Dr. Bence Jones on 51 109
—— Dr. Fuller on 48 1
—— Dr. Fuller on the treatment of .. .. 47 xii
—— Dr. Ross on its connexion with some eruptive skin diseases .. 47 14
—— Dr. Ross on the treatment of skin diseases caused by 48 139
—— chronic, Dr. Duncan on dilute hydrochloric acid in .. 51 xii
—— on the means of detecting 51 112
Gouty, MM. Charcot and Cornil on the condition of the kidney in the 51 108
——— deposits, Dr. Harley on 50 86
Graefe's operation for strabismus, Mr. Soelberg Wells' description of 47 238
Granular conjunctivitis, Mr. Bader on its treatment by inoculation .. 50 220
——— lids and vascular corneæ treated by purulent inoculation 52 275
——— ophthalmia, Mr. Lawson on its treatment by inoculation .. 50 229
Gravel, Dr. Bence Jones on 51 127
——— and calculi, phosphatic, Dr. Harley on 51 136
Guide-hook, Mr. Newham's, a new obstetric instrument 51 242
Gurgina balsam, or wood oil, in gonorrhœa 51 328
Gymnastic exercise, its efficacy in chronic convulsive disorder .. 48 25

	VOL.	PAGE.
Hæmostatic agent, on the employment of acupressure as a	52	171
Hair, Dr. Frodsham on the treatment of alopecia, or falling off of the	50	186
Hare-lip, Prof. Fergusson on the operation for	50	132
——— Mr. Bryant on	48	110
——— Mr. Butcher's operation on an unusual form of	48	103
——— operation, Mr. Smith's instrument for controlling hemorrhage in	51	212
Hair-pin, extraction of from the urinary bladder	49	333
Head, Dr. Ashhurst on injuries of the	50	128
Headaches of nervous women, employment of essence of turpentine in	50	373
Heart, Dr. Flint's account of valvular lesions of the	47	69
——— Dr. Kennedy on fatty disease of the	50	44
——— M. Decaisne on the effect of tobacco on the	50	50
——— diseases, Dr. Fuller on the action of digitalis in	47	106
——— diseases, Dr. Gull on the action of digitalis in	52	91
——— diseases, Dr Henry on the value of digitalis in	52	99
——— diseases, Dr. Leared on the action of digitalis in	47	105
——— diseases, Dr. Murray on the action of digitalis in	52	98
——— enfeebled, on the value of digitalis in	47	101
Hematocele, pelvic, Dr. M'Clintock on	48	264
Hemorrhage, Prof. Macnamara on acupressure for controlling	52	180
——— Dr. Smith's instrument for controlling in hare-lip operations	51	212
——— Dr. Wise on the hemostatic treatment of	48	18
——— Mr. Churchill's new application of the ligature for	52	185
——— Mr. Porter on a new mode of arresting after amputation	49	124
——— post-partum, Dr. Earle on the scientific treatment of	50	310
——— post-partum, Mr. Macdonald on the varieties of	47	278
——— post-mortem, Mr. Parsons on electro-magnetism in	52	355
——— uterine, from fibrous tumours or polypi, Mr. Brown on	50	314
Hemorrhoids, Dr. Cartwright on the efficacy of persulphate of iron in	50	157
——— Mr. Allingham on the treatment of	52	190
——— Mr. Cartwright on persulphate of iron in	52	203
——— Mr. Henry Smith's treatment of	47	187
——— Mr. Lane on the advantages of the ligature in	52	193
——— Mr. Smith on the clamp operation for	52	202
——— Mr. Turner on the use of carbolic acid in	48	368
——— and prolapsus, Mr. H. Smith on the use of clamp in	48	117
——— discussion on the methods of treatment of	47	188
Hepatic diseases in the East Indies, Sir Ranald Martin on	50	74
Hepatitis, acute, Dr. Cameron on the treatment of	48	69
Hernia, Mr. Jordan on the extra-peritoneal operation for	50	156
——— Mr. Jordan on a simplification of the extra-peritoneal operation in	50	148
——— Mr Kingdon on the causes of	50	156
——— Mr. Porter on the advantage of Syme's operation for the cure of	49	144
——— Mr. Salt's new form of truss for	49	154
——— Mr. Wood's new truss pads for	49	146
——— Mr. Wood's operation for the radical cure of	48	386
——— Mr. Wood's operation for radical cure of	49	392
——— Mr. Wood's operations for the cure of	49	150
——— inguinal, Dr. Fayrer on the radical cure of	52	187
——— inguinal, Mr. Dickinson on Prof. Chisholm's operation for	48	115
——— strangulated femoral, Mr Prescott Hewett on	47	182
——— strangulated, its reduction after failure of taxis	47	184
Herpes circinatus, Dr. Lane's treatment of	47	217
Hip-joint, Mr. Spence's case of amputation at the	47	144
——— disease, Mr. Barwell on the history and treatment of	47	139
——— disease, Mr. Barwell on the treatment of	48	384
——— disease, Mr. Barwell's splint for the treatment of	48	83
Hooping-cough, Dr. Fuller on belladonna and sulphate of zinc in	48	320
——— Dr. Gibb on bromide of ammonium in	48	321
——— Dr. Harley on the efficacy of bromide of ammonium in	49	76
——— Dr. Ritchie on bromide of ammonium in	50	57
——— M. Guerard on gas fumes in	50	60
——— Mr. Garraway on belladonna and sulphate of zinc in	48	318
Horsehair as a substitute for wire-sutures	47	167

	VOL.	PAGE.
Hospital gangrene, Mr. Maunder on	50	209
Hydatid disease affecting the kidneys, Dr. Adams's case of	50	93
Hydrocele, Dr. Jollie on the treatment of	51	251
——— Dr Thompson on acupuncture in	51	250
Hydrochlorate of morphia, Dr. Garrod on..	49	363
Hydrochloric acid, its use in consumption	50	52
——— acid, its use in the treatment of dysentery	48	68
Hydrocyanic acid, its use in the treatment of insanity	47	65
Hydrophobia, Dr. Lassing on the use of electricity in	47	53
Hypertrophy of the prostate, Mr H. Thompson on catheterism in ..	48	124
Hyperæsthesia and prurigo, connexion between gout and	47	14
Hypertrophic elongation of the cervix uteri—amputation	47	344
Hypodermic injections, Prof. Nassbaum on the danger of ..	52	91
——— injection of morphia, Mr. Freeman on the ..	52	76
——— treatment of disease, Mr. Hunter on the progress of	52	82
Hypophosphites in neuralgia, Dr. Jackson on	52	70
——— in remittent fever, &c., Dr. Purdon on ..	52	6
Hysteria, Mr. Ellis on the efficacy of electricity in an inveterate case of	47	352
——— Mr. Skey on	49	335
Hysterotomy, Mr. Spencer Wells on	52	337
Hysterical chorea, its treatment by sulphate of aniline	51	53
——— paralysis, Dr. Savage on its treatment by galvanism ..	51	50
——— paraplegia, Dr. Gull on the efficacy of electricity in ..	47	345

Ice to the spine, case of epilepsy with insanity cured by	52	64
— to the spine, case of traumatic tetanus treated by	50	35
Ileus, with fæcal vomiting, Dr. Stokes's case treated by galvanism	51	102
Imperfect deliverance in abortion or labour, some results of	47	339
Impetigo contagiosa, or porrigo, Dr. Fox on the treatment of ..	50	190
Impregnation, Dr. Packman on	48	227
Incontinence of urine in boys, Mr. Adams on	50	389
Infantile strophulus, Dr. Martin's treatment of	52	220
——— syphilis, Mr Solomon's case of from secondary contagion ..	50	290
Infection, purulent, on the etiology of	47	297
Inflammation, Dr. Hughes Bennett on	48	xi
Inflammation as an exciting cause of tuberculosis, Dr. Jones on ..	51	58
Inguinal hernia, Dr. Fayrer on the radical cure of	52	187
——— hernia treated successfully by Prof. Chisholm's method ..	48	115
Injections of alum in gonorrhœa	47	226
——— vaginal, Dr. Rasch's instrument for	51	394
Inhalation of atomised fluids, Dr. Beigel on the	52	102
Inherited syphilis, Mr. Hutchinson on the signs of	48	224
Injuries of the head, Dr. Ashhurst on	50	128
Inoculation, Mr. Lawson on the treatment of granular eyelids by ..	50	229
Insanity, Dr. Mc'Leod on hydrocyanic acid in the treatment of ..	47	65
——— Dr. Newington on a new remedy in	52	72
——— Dr. Robertson on the use of digitalis in	49	46
Insomnia, Dr. Hammond on the treatment of	52	74
Intermittent fever, Dr. Ward on the treatment of	50	20
Intra-cranial amaurosis, Mr. Hutchinson on	51	299
Intussusception, Dr. Greig on inflation as a remedy in	50	68
Inversion of the uterus, Dr. Kelly on	49	264
——— of the uterus, Mr. Sheppard on	49	268
Iodide of iron, its use in consumption	50	52
——— of potassium, Dr. Salter on its use in asthma	49	71
——— of potassium as an antilactescent, Dr. Morris on	50	329
——— of potassium in the treatment of aneurism	47	171
Iodine, Dr. Prieur on its use in inflamed glands	51	276
——— as a disinfectant, Mr. Druitt on	49	377

	VOL.	PAGE.
Iodine, as a dedoriser and disinfectant	48	356
—— tincture of, its use in arthritis	48	132
Iridectomy, Mr. Bowman on	49	221
—— Mr. Browne on	49	235
—— Mr. Critchett on	49	211
—— Mr. Hildige on	49	238
—— Mr. Hogg on	49	237
—— Mr. Walton on	49	213
—— Mr. Wharton Jones on the operation of	50	257
—— and section of the ciliary muscle, Ed. of Brit. Ophthalmology on	50	252
—— as a means of treating glaucoma, Mr. Wharton Jones on	50	268
—— in destructive ulceration of the cornea	48	192
Iris, prolapsed, Mr. Hart's case of	49	209
—— prolapsed, Mr. Lawson on the treatment of	51	282
—— prolapsed, Mr. Nunneley on the use of the Calabar bean in ..	48	203
Iridesis, Mr. Critchett's case of congenital cataract treated by ..	50	246
Iritis, Dr. Lawrence on its treatment by morphia	47	245
—— Mr. Hogg on the value of belladonna in	50	235
—— use of the Calabar bean in	48	201
Irrigation in penetrating wounds of the knee-joint	51	177
Irritable bladder, Dr. Meade on the use of citrate of ammonia in ..	48	81
—— bladder, Mr. Thompson on the use of triticum repens in ..	47	132
—— stomach, anæsthetic action of the Calabar bean in ..	48	200
—— uterus, Dr. Griffith on bromide of ammonium in	49	330
Itch, Dr. Gale's treatment of	50	194
—— Dr. Hardy on the treatment of	50	193
—— M. Gudden's recent researches on	52	251
—— M. Hardy's rapid cure of	52	258
—— on the treatment of	51	274
Jaundice, Dr. Gull's case of carcinoma about the gall-ducts causing	50	79
—— Dr. Harley on administration of podophyllin in	47	119
Jaws, Prof. Fergusson on operations on the	52	147
Joint diseases, Mr. Barwell on certain errors in the treatment of ..	52	130
Joints, Dr. Monro on severe wounds penetrating into	50	110
—— distended, Prof. Inzani on the importance of tapping ..	48	385
Kamela and male-fern combined, efficacy of in tape-worm ..	51	106
Keratitis, phlyctenular, Mr. Watson on the use of the seton in ..	51	281
Kidney, Dr. Hassall on Bright's disease of the	51	148
—— Dr. Jenner on Bright's disease of the	51	142
—— on its condition in the gouty	51	108
Knee-joint, Prof. Fergusson on excision of the	50	99
—— Dr. Barton on excision of the	51	174
—— Dr. Newman on irrigation in penetrating wounds of the	51	177
—— Mr. Butcher on excision of the	47	137
—— Mr. Butcher on excision of the	48	86
—— Mr. Hilton on importance of mechanical rest in disease of the	47	134
—— Mr. Le Gros Clark on resection of the	49	96
—— Mr. Smith on resection of, in children	48	90
Labour, Dr. Barnes on arresting flooding after	51	349
—— Dr. Granville on extreme cold as an anodyne to pain in ..	51	341
—— Dr. Graily Hewitt on the advantages of turning in.. ..	50	294
—— Dr. Inglis on the facilitation of the first stage of	52	318

	VOL.	PAGE
Labour, Dr. Lawrence on the use of the forceps in	47	271
—— Dr. Munroe on a rare case of	49	276
—— Dr. Palfrey's case in which uterine action progressed during sleep	49	275
—— Mr. Paterson on supporting the perineum during	51	335
—— Mr. Pridham on a suddenly fatal case after	47	269
—— Mr. Rigden's case of inversion of the uterus after	51	344
—— natural, Dr. Paterson on the management of the placenta in	52	333
—— on the injurious effects of chloroform inhalation during ..	48	243
—— premature, Prof Giordano on its induction by nitrate of silver	48	234
—— premature, Dr. Barnes' case of induction of	47	259
—— premature, Dr. Keiller on inducing with cautchouc bags ..	48	228
—— premature, Dr. Ramsbotham on its induction by ergot ..	48	233
—— premature, Mr. Corner's case of induction by Dr. Barnes' method	47	260
—— premature, Mr Porter's case of induction by Dr. Barnes's method	50	293
—— premature, induction of in a case of deformed pelvis .	52	327
—— protracted, Dr. Murphy on delivery of the child in	48	247
—— tables of mortality in natural protracted labours and in forceps cases	48	250
—— tedious, Dr. Brown on the use of ergot in	52	324
Lacerated perineum, Mr. Bryant on the operation for	52	368
—— perineum, Mr. Lane on the operation for	52	370
Laceration of the perineum, prevention of in forceps cases	47	275
Lachrymal apparatus, Mr. Critchett on diseases of the	49	189
—— obstructions, Mr. Bowman's treatment of	49	194
—— duct, Mr. Haynes Walton on	48	184
Lactucarium, Dr. Garrod on	49	367
Laryngismus stridulus, Dr. Ramskill's treatment of	49	63
Laryngoscope, Dr. Johnson's suggestions for the use of the	47	109
—— Dr Sieveking on the use of the	51	91
—— Mr. Mason's improved	48	101
Laryngoscopy, Dr. Johnson's suggestion in aid of	52	106
Larynx, glycerine as a vehicle for local applications to the	50	148
Lead poisoning, from spring water, Dr. Osborn on .. .	50	xxxv
Lepra, Dr. M'Call Anderson on the treatment of	52	237
—— and psoriasis, Mr. Nayler on the treatment of	48	152
Lichen and eczema complicated with gout	47	16
Liebig's soup for children	51	xl
Ligature, Prof. Fergusson on the advantages of the	47	162
—— Mr. Churchill's new mode of applying the	52	185
—— Mr. Dix on the wire compress as a substitute for the ..	47	158
—— Mr. Dix on the wire compress as a substitute for the ..	50	113
—— Mr. Lane on its advantages in internal hemorrhoids ..	52	193
—— silver-wire Mr. Davies on	47	164
—— wire, Mr. Holt's cases in which it was left in the wound ..	50	125
Liquor bismuthi, Mr. Schacht's preparation of	48	342
Lithotomy, Dr. Humphry's method of performing	49	155
—— Mr. Holmes Coote on the operation of	51	229
—— Mr. Hutchinson's tube for use after	50	168
—— Mr. Lane on the operation of	51	244
—— Mr. Thompson on the median operation of	49	166
—— advantages of the straight over the curved staff in ..	51	245
—— in children, Prof. Fergusson on	50	157
—— in the adult, Prof. Fergusson on	52	209
—— in the female, Dr. Humphry on	50	172
—— in the female, Mr. Lane's operation for	47	210
—— vaginal, Dr. Aveling and Mr. Spencer Wells on ..	47	215
Lithotrity, Prof. Fergusson on	52	203
—— Prof. Fergusson on	50	161
—— Mr. Holmes Coote on	51	231
—— Mr. H. Thompson on its peculiar applicability in paralysed bladder	51	242
—— Mr. H. Thompson on the eminently successful results of ..	51	232
—— Mr. H. Thompson on the instruments employed in ..	51	239
—— Mr. H. Thompson on the successful performance of ..	51	236
—— without injections, Mr. Pollock on	49	161
—— without injections, Mr. Teale on	49	165
—— without injections, Mr. Thompson on	49	159

	VOL.	PAGE.
Liver, Dr. Pavy on the assimilation of sugar by the	52	113
—— Dr. Wilks on syphilitic formations in the	48	220
—— inflammation of, as it prevails in the East Indies	50	75
London paste as an escharotic, Dr. Mackenzie on	50	143
Long bones, Mr. Hutchinson on separation of the epiphyses of ..	50	131
Lunar caustic, M. Brou on a new mode of preparing	47	xxxviii
Lying-in women, Dr. Graily Hewitt on the diet of	50	324
Malarious fever, Mr. Hare on the treatment of	51	24
Male-fern oil, its efficacy in tapeworm	47	112
———— its efficacy in tapeworm	52	119
———— method of exhibiting in tapeworm	47	116
Mammary tumours, Mr. Bryant on	50	374
Manganese as a remedy for pain caused by food, Dr Leared on ..	51	98
Measles, Dr. Kennedy on a disease resembling	47	2
Meatus and tympanum, Mr. Hinton's treatment of catarrh of the ..	49	250
——— urinarius, Dr. Griffith on cases of vascular tumour of the ..	49	358
Mechanical rest in diseases of the knee-joint, importance of ..	47	131
Medicines, Dr. Nelson on the action of	48	376
Melancholia, Mr. Hunter on the hypodermic treatment of ..	52	86
Menorrhagia, Dr. Graily Hewitt on ergot of rye in	47	353
Menses, retained, of two year s duration, from atresia vaginæ ..	48	295
Menstruation, painful, Dr. Henry Bennet on the treatment of .	52	364
Mentagra, M. Bazin on the treatment of	49	173
Mercurial fumigation in syphilis, Mr. Fleischmann on	48	222
——— ointments in the treatment of psoriasis	52	243
——— vapour, Mr. Parker on the treatment of syphilis by ..	50	289
——— vapour, Mr. Pollard on the treatment of syphilis by ..	50	284
Mercurialised collodion as an application to condylomata	51	326
Mercury, Mr. Dunn's cases of syphilis treated without	51	226
——— in the treatment of syphilis	51	313
——— in the treatment of syphilitic eruptions	47	234
Milk, Dr. Morris on iodide of potassium to arrest the secretion of ..	50	329
—— Dr. Skinner on the use of Faradization to promote the flow of	49	279
—— fever, Dr. Graily Hewitt on..	50	325
Morbus coxæ, Mr. Barwell on the treatment of	47	139
Morphia, Dr. Dupre on the iodic test for	48	359
——— Mr. Freeman on the hypodermic injection of	52	76
——— hydrochlorate of, Dr. Garrod on the	49	363
——— subcutaneous injection of in cholera	47	117
——— treatment of sclerotitis and iritis	47	249
Mortality, early, Dr. Whitehead on the causes of	47	355
Muscles, effects of poisons on	47	377
Muscular atrophy, Dr. Radcliffe on	51	44
——— pain, formula of a local anæsthetic for relief of	50	43
——— rheumatism, treated by the local application of chloroform	50	42
Mydriasis, Mr. Hart's case treated by solution of Calabar bean ..	48	204
——— use of the Calabar bean in	48	201
Nævi, large subcutaneous, Mr. Murray on their removal by ligature ...	49	120
Nævus, Dr. Zeissl on the use of tartar emetic in	47	178
——— Mr. Carter's case of death following subcutaneous injection of a	48	99
——— of the eyelid, Mr. Wordsworth on puncture with a hot needle	47	179
Nails, ingrowing, Dr. Billon's treatment of	48	143
Nasal cavity, Dr. Skinner on a new mode of treating diseases of ..	51	204
—— cavity, Dr. Thudichum on a new mode of treating diseases of ..	51	196
—— duct, operation of catheterizing the	49	195

	VOL.	PAGE.
Nelaton's porcelain probe for discovering balls in wounds	47	169
—— probe, Dr Hamilton on, with case	49	103
Neuralgia, Prof. Courty on the employment of local injections in ..	50	42
—— Dr. Cahen on its treatment by arsenical preparations ..	49	67
—— Dr. Dupuy on the local application of chloroform in ..	50	42
—— Dr. Garrod on the external employment of aconitine in ..	49	67
—— M. Trousseau on	50	1
—— and hypophosphites, Dr. Jackson on	52	70
—— facial, Mr. Craig on acupuncture in	50	38
Neuralgic affections, Dr. Braithwaite on the efficacy of gelseminum in	49	68
—— affections, Dr. Sewell on croton oil in	52	67
—— affections, anæsthetic action of the Calabar bean in ..	48	200
Nervous headache, Prof. Teissier on internal use of essence of turpentine in	50	373
Nicotine, Dr. Barton on a case of traumatic tetanus treated by ..	48	34
—— Dr. Ogle's case of traumatic tetanus treated by	49	53
—— its use in the treatment of traumatic tetanus	47	46
Nipples, sore, Dr. Castle's treatment of	48	272
—— sore, treatment of	51	361
Nitrate of potash in the treatment of acute rheumatism	47	18
—— of silver, Mr. Ellis on its properties, and instrument for using	48	312
—— of silver, Mr. Higginbottom on the new preparations of ..	48	354
—— of silver, its use to prevent pitting after small-pox	48	164
Nitric acid, its efficacy in prolapsus ani of forty years' standing ..	47	186
—— its use in the treatment of hemorrhoids	47	187
Nitro-benzine and aniline, their different effects on the system ..	51	55
—— benzole and aniline, Dr Letheby on the physiological properties of	48	388
Nose, Dr. Cahill's method of plugging in epistaxis	47	181
—— on the medicinal solutions which may be applied to the cavity of	51	201

Obscure disease, importance of urinary analysis in cases of ..	50	92
Obstetric forceps, Dr. Gayton's, and mode of securing the handles ..	48	251
—— guide-hook, Mr. Newham's	51	342
Obstruction of the eustachian tubes, treatment of	49	242
Occipito-posterior position, Dr. Leishman on rectification of ..	49	269
Ocular paralysis, Dr. Benedikt on its treatment by electricity ..	52	301
Oil of male-fern, its efficacy in tapeworm	47	112
—— male-fern, in tapeworm, mode of exhibition of	47	116
—— yellow Sandal wood, its use in gonorrhœa	51	328
Onanism, Mr. Hilton on the treatment of	48	137
Opium, Dr. Garrod on the preparations of	49	363
—— M. Claude Bernard and Dr. Ozanane on the alkaloids of .	51	xxxix
Ophthalmia, catarrhal, Dr. Henderson on	48	191
—— catarrhal, Mr. Hancock on the treatment of	52	265
—— gonorrhœal, Dr. Collis' treatment of	47	257
—— purulent, Mr. Hart on the treatment of	48	189
—— purulent, Mr. Wordsworth on its treatment in infants..	48	188
—— strumous, Dr Price on the use of tartarised antimony in	50	228
Ophthalmic diseases, Dr. Robertson on the use of the Calabar bean in	48	202
Opium as a remedy in some cases of epilepsy	48	28
—— poisoning, Dr. Hatrick on the treatment of..	48	345
—— poisoning, Dr. Ogle on galvanism in	48	352
—— poisoning treated by belladonna	47	377
Optic atrophy, Mr. Hart on smoking as a cause of	48	213
Orchitis, acute, Mr. H. Smith on puncturing the testicle in ..	50	183
Osteo-aneurism, Dr. Mapother's case of	47	177
Ovarian dropsy, Drs. Warren and Storer's case of	47	335
—— dropsy and ascites, Mr. Spencer Wells on the diagnosis between	50	362
—— tumour, Mr. Henry Smith's case of	48	291
—— tumours, Dr. Clay's cases of extirpation of	50	356
—— tumours, Dr. Gairdner's cases of uterine tumours simulating	50	342

	VOL.	PAGE.
Ovariotomy, Prof Simpson on the application of acupressure in	49	304
—— Dr. Babington on	47	323
—— Dr. Clay's paper on	47	308
—— Dr. Clay on the critical days in	49	289
—— Dr. Clay on the large incision in	49	288
—— Dr. Clay on the ligatures used in	49	288
—— Dr. Clay on the operation in respect to age	49	290
—— Dr. Clay on the results of his cases of	49	286
—— Dr. Clay on the use of chloroform in	49	288
—— Dr. Clay on the use of purgatives in the after treatment	49	290
—— Dr. Clay's results of his operations	51	367
—— Dr. Graily Hewitt on	47	314
—— Dr. Robert Lee's paper on	47	323
—— Dr. Robert Lee on	47	329
—— Dr. Parson on the administration of chloroform in	51	374
—— Dr Savage on	47	324
—— Dr Scott's case of	51	378
—— Dr Tyler Smith on	47	323
—— Drs. Smith, Wells, and Hicks on securing the pedicle in	51	368
—— Editor of Lancet on	47	334
—— Mr. Baker Brown on	47	309
—— Mr. Baker Brown on the operation of	49	296
—— Mr Baker Brown on the operation and its results	48	298
—— Mr. Brown's new method of securing the pedicle in	51	370
—— Mr Bryant on the operation of	51	371
—— Mr. Erichsen on	47	316
—— Mr. Hutchinson on	47	319
—— Mr. Keith's case of	52	383
—— Mr. Spencer Wells' paper on the history and progress of	47	313
—— Mr. Spencer Wells on	47	311
—— Mr. Spencer Wells on	47	319
—— Mr. Spencer Wells on	47	326
—— Mr. Spencer Wells on the operation of	48	306
—— Mr. Spencer Wells on practical details in	50	368
—— in a woman aged 65. Mr. Hutchinson's successful case of	47	321
Oxalic acid diathesis, Dr. Bence Jones on the treatment of	51	125
Oxygen, ammonia, and bromoform, inhalation of in epilepsy, &c.	48	33
Oxytoxic, Dr. Parker on tartrate of antimony as an	47	265
Ozœna, use of permanganate of potash for removing the fetor of	51	198
—— chronic, Dr. Skinner's treatment of	51	204
Ozone, Dr. T. Moffatt on	52	19
Pain caused by food, Dr. Leared on its treatment by manganese	51	98
—— in the back, chronic, Dr. Gervis on a variety of	49	347
—— of the spine, Dr. Dick's cases of	52	61
Painful spasmodic affections, Mr. Hunter on the hypodermic treatment of	52	88
Palsy, scriveners', or the paralysis of writers, Mr. Solly on	51	41
Pannus, Dr. Bader on the operation of syndectomy for	48	177
—— Mr. Bader on its treatment by inoculation with pus	50	220
Paralysed bladder, Mr. H. Thompson on the applicability of lithotrity in	51	242
Paralysis, Dr. Althaus on galvanization and faradisation in	52	46
—— Prof. Courty on the use of local injections in	50	42
—— general hysterical, treated by the continuous galvanic current	51	50
—— local, Mr Paget on the treatment of cases of	49	64
—— of the hand and forearm from smoking, Dr. Banks's case of	50	44
—— of the ocular muscles, Dr. Benedikt's treatment of	52	301
—— reflex, Dr. Robertson on	52	42
Paracentesis of the cornea, Mr. Bowman on	49	225
—— of the cornea, Mr. Walton on	49	216
Paraplegia, puerperal, good effects of electricity in	47	345
Parasitic affections of the skin, Mr. Wilson on the treatment of	49	174

	VOL.	PAGE.
Parturition, Dr. Donkin on the physiological action of the uterus in	49	338
——— Dr. Granville on extreme cold as an anodyne in the pain of	51	341
Pelvic hematocele, Dr. M'Clintock on	48	264
Pepsine as a medicinal agent	47	131
Perchloride of iron as a means of arresting flooding	51	349
——— of iron, case of death following subcutaneous injection of	48	100
——— of iron, its action in renal and urinary affections	51	150
Perforation, Dr. Hicks on the best mode of delivering the fœtal head after	51	345
Pericarditis, Mr. Hilton on the influence of physiological rest in	48	41
——— consequent on pyæmia, Dr. Kirkes on	47	93
——— rheumatic, Dr. Fuller on	47	88
Perineum, Mr. Paterson on supporting during labour	51	335
——— lacerated, Mr. Bryant on the treatment of	52	368
——— lacerated, Mr. Lane on the operation for	52	370
——— ruptured, Mr. Brown on the operation for	49	323
Peritomy, operation of, in vascular corneæ	48	176
Peritonitis, Mr. Hunter on the hypodermic treatment of	52	90
——— puerperal, Mr. Mitchell on the treatment of	48	284
Permanganates, alkaline, as antidotes to organic poisons	47	371
Persulphate of iron in hemorrhoids, Dr. Cartwright on	50	157
——— of iron in hemorrhoids	52	203
Pessary, uterine, Dr. Wilkie's double-jointed	48	307
Pessaries, Dr. Clay on wire loops as substitutes for	49	253
——— on the use of, in retroversion of the uterus	48	260
——— uterine, Dr. Tanner on the use of medicated	48	310
Phenic acid, on the action of	48	xlvi
Phlegmasia dolens, Dr. Barnes on	52	346
——— dolens, Dr. Graily Hewitt on	50	327
Phlyctenular keratitis, Mr. Watson on the use of the seton in	51	281
Phosphatic gravel and calculi, Dr. Harley on	51	136
Phosphorus as a disinfectant	52	19
——— its efficacy in epilepsy and chronic convulsive disorders	48	26
——— its use in consumption	50	51
Phthisis, Dr. Blake on the influence of living in the open air in	50	56
——— Dr. Cotton on the value of different therapeutic agents in	50	51
——— Dr. Russell on fever and bronchitis as early signs of	51	68
Physiological researches of the Rev. Professor Haughton	47	363
Phytodermatous furfuræ, treatment of	49	173
Pitting after small-pox, Dr. Bowen on the prevention of	48	164
——— after small-pox, Mr. Higginbottom on the prevention of	48	165
——— in small-pox, on the prevention of	47	222
Pityriasis Mr. Nayler on the treatment of	49	176
Placenta, Dr. Paterson on the management of in natural labour	52	333
——— prævia, Dr. Murphy on the treatment of	48	246
——— prævia, Dr. Trask on	52	323
——— prævia, Dr. Williams's case of	48	247
Plasters, ready-made, Dr. Tilt on	49	353
Plugging the nose in severe cases of epistaxis	47	181
Pneumonia, Dr. Beale on depletion and excessive stimulation in	48	17
——— Dr. Bennett on its treatment by restoratives	51	75
——— Dr. Waters on auscultatory signs which precede crepitation in	51	73
——— acute, Dr. Bennett on	47	107
Podophyllin, mode of exhibition in jaundice	47	119
Poisons, detection of in medico-legal cases by dialysis	47	366
——— effects of on muscles	47	377
——— organic the alkaline permanganates as antidotes for	47	371
Poisoning, Dr. Hatrick on the treatment of cases of	48	345
——— by belladonna, Dr. Fraser's treatment of	50	xxxvi
——— by chloroformic anodyne, Dr. Harley on	48	343
——— by corrosive sublimate, Dr. Buckler's treatment of	48	347
——— by cyanide of potassium, Dr. Davanne on	48	353
——— by morphia, use of caffeine in	47	xxxviii
——— by opium, Dr. Ogle on galvanism in	48	352
——— by opium, Dr. Morris on its treatment by belladonna	47	377

	VOL.	PAGE.
Poisoning by veratrum viride	47	372
Politzer's eustachian catheter	49	248
Polycystic disease of right ovary with ulceration, Mr Henry Smith on	48	291
Polypus of the rectum, with anal fissure, Mr. Lane on ..	52	200
——— of the rectum in children, M. Guersant on	50	383
——— uteri, Prof. Simon's method of extirpating large	48	291
——— uteri, Dr. Marion Sims on the écraseur for removal of ..	51	381
Popliteal artery, digital pressure in aneurism of the	52	183
Porrigo, Dr. Fox on the treatment of	50	190
Porte caustique, uterine, Dr. Lente's	49	346
Potassium, chloride, bromide, and iodide of, properties of	49	379
Pregnancy, Dr. Bell on abortion in the early months of ..	52	329
——— Dr. Cassells on the sickness of	51	334
——— Mr. Pennefather's case of coexisting extra- and intra- uterine	48	252
——— early, Dr. Routh on some of the symptoms of ..	51	331
Premature labour, Dr Barnes' method of inducing..	47	259
——— labour, Dr. Graily Hewitt on the induction of ..	52	327
——— labour, Dr. Keiller on its induction by caoutchouc bags ..	48	228
——— labour, Dr. Ramsbotham on its induction by ergot ..	48	233
——— labour, Mr. Corner's case of induction by Dr. Barnes' method	47	260
——— labour, Mr. Porter's case of induction by Dr. Barnes's method	50	293
——— labour induced by means of nitrate of silver	48	234
Presentation of right arm and shoulder, delivery by spontaneous evolution	48	255
Pre-taxoid operation for strangulated hernia, Mr. Jordan's ..	50	151
Procidentia uteri, Dr. Emmet on a radical operation for ..	52	374
Profuse perspiration, Dr. Smith on hot water as a remedy for ..	51	71
Progressive locomotor ataxy, Dr. Radcliffe on	52	53
Prolapse of the funis, Dr. Earle on the treatment of	49	277
——— of the bladder, with distension, in the first stage of labour ..	49	350
——— of the iris, Mr. Hart's case of	49	209
——— of the iris, Mr. Lawson's treatment of	51	282
——— of the iris, use of the alkaloid of the Calabar bean in ..	48	203
——— of the uterus, bladder, and rectum, Mr. Brown on	49	322
Prolapsus ani, Mr. Holthouse's case treated by nitric acid	47	186
——— ani, Mr. H. Smith on the use of the clamp in	48	118
——— ani, of the rectum, Mr. H. Smith's new clamp for ..	47	187
——— uteri, Dr. Emmet on a radical operation for ..	52	374
——— uteri, Dr. Greenhalgh on the treatment of	48	262
——— uteri, Mr. Davies on the treatment of	49	327
Prostate, hypertrophy of, Mr. H Thompson on catheterism in ..	48	124
Protracted labour, Dr. Murphy on delivery of the child in	48	247
Prurigo, Dr. Duchesne-Duparc on the treatment of	50	191
——— Mr. Hardy's treatment of	52	221
——— Mr. Milton on the treatment of	52	217
Pruritus pudendi, M. Dieudonne's treatment of	51	xxxviii
Psoriasis, Dr. M'Call Anderson on the treatment of	52	237
——— Dr. Vingtrinier's treatment of	50	193
——— and lepra, Mr. Nayler on the treatment of	48	152
——— consequent on gout	47	16
——— tar ointment as a local application in	48	141
——— with gouty diathesis, treatment of	48	140
Puerperal convulsions, Dr. Kidd on the use of chloroform in.. ..	49	354
——— convulsions, Dr. M'Nab on chloroform in	48	236
——— convulsions, Mr. Jones on the treatment of	48	234
——— convulsions, Mr. Prosser's cases treated with and without bleeding	48	241
——— convulsions successfully treated by chloroform, Dr. Cuthill on	52	348
——— diseases, Dr. Graily Hewitt on	50	326
——— fever, Dr. Barnes on the constitutional and local causes of	51	349
——— fever, Dr. Barnes on	52	343
——— fever, Dr. Latham on	47	294
——— fever, M. Grisar on its propagation by accoucheurs ..	51	360
——— fever, Dr. Snow Beck on	52	339
——— infection, M. Flourens on	47	297
——— patients, Prof. Simpson on eruptive fevers in	48	270

	VOL.	PAGE.
Puerperal peritonitis, Mr. Mitchell on the nature and treatment of	48	282
——— pyæmia, Dr. Barnes' case of	47	306
——— tetanus, Dr. Philipson on	52	347
——— tetanus following abortion, Dr. Blackshaw's case of	52	350
——— thrombosis and embolia, Dr. Barnes on	52	345
Pulmonary affections, on the action of expectorant remedies in	48	62
Puncta and canaliculi, Mr. Critchett on abnormal conditions of the	49	190
Pupil, Dr. Ogle on some effects of Calabar bean on the	48	206
——— effects of solution of the Calabar bean on the	48	195
——— artificial, Mr. Solomon's case of	51	296
——— power of the Calabar bean in contracting	48	201
Purulent infection, Prof. Polli on the sulphites and hyposulphites in	49	9
——— ophthalmia, Mr. Hart on the treatment of	48	189
——— ophthalmia in infants, Mr. Wordsworth on	48	183
Putrid dead body, attempt to restore the natural appearance of a	48	336
Pyæmia pericarditis consequent on	47	93
——— puerperal, Dr. Barnes' case of	47	306

	VOL.	PAGE.
Quinine, Mr. Walker on the therapeutical action of	47	xxxix
——— as an antidote in malarious fever	51	24
——— sulphate of, to detect the adulteration of	48	343

	VOL.	PAGE.
Ranula, Mr. Skey on the treatment of	49	188
Ready-made plasters, Dr. Tilt on the preparation of	49	353
Rectum, Mr. Curling's case of painful cancer of the	51	223
——— Mr. Lane on polypoid growths in the	52	200
——— polypus of, in children, M. Guersant on	50	383
——— prolapsus of, Mr. Smith's treatment of	47	187
Recurring fibroid tumour, Mr. Bryant on the	48	23
Reflex disease of the eye of traumatic origin	50	275
——— paralysis, Dr. Robertson's cases of	52	42
Remittent fever, Dr. Purdon on the hypophosphites in	52	6
Renal affections, Dr. Hassall on the action of perchloride of iron in	51	150
Resection of the knee-joint, Mr. Le Gros Clark on	49	96
Retained menses from atresia vaginæ, Mr. Brown's case of	48	295
Retention of urine, Mr. Holt on the diagnosis and treatment of	47	192
——— of urine in cases of tight stricture, new catheter for	48	122
Retroflexion of the uterus, Mr. Baker Brown's treatment of	50	351
Retroversion of the uterus, Dr. Clay on	48	260
Rheumatic affections, Dr. Sewell on croton oil in	52	67
——— affections, scarlet fever complicated with	47	1
——— bronchitis, Dr. Jones's treatment of	51	80
——— fever, Dr. Davies on the blister treatment of	51	36
——— fever, Dr. Dobell on the germs of	52	37
——— fever, Dr. Nevins on the treatment of	48	9
——— fever, Mr. Everett on the use of alkaline baths in	51	40
——— fever, Mr. Roberton on the treatment of	50	21
——— fever, sedative action of the Calabar bean in	48	200
——— gout, Dr. Fuller on	48	1
——— pericarditis, Dr. Fuller on the treatment of	47	88
Rheumatism, Dr. Fuller on cases simulating	47	32
——— acute, Dr Basham on nitrate of potash in	47	18
——— acute, Dr. Davies on the blister treatment of	52	39
——— acute, Dr. Davies on its treatment by blisters	50	xi
——— acute, Dr. Fuller on the treatment of	47	24

	VOL.	PAGE.
Rheumatism, acute, Dr. Inman on the effiacy of lime-juice in	48	14
———— gonorrhœal, Dr. Fuller on	47	34
———— gonorrhœal, Dr. Popham's case of	49	16
———— muscular, Dr. Dupuy on the local use of chloroform in	50	42
———— tendinous, Dr. Renard on the use of sulphur in	49	19
Ringworm, Dr. Lane's treatment of	47	217
Royal Humane Society's instructions for restoring the apparently dead	48	340
Rupture, Mr. Wood's operations for the cure of	49	150
——— Mr Wood on the radical cure of	49	392
——— congenital, Mr. Vines on a simple support for	47	185
——— of the uterus, Dr. Crighton on gastrotomy in	50	304
——— of the uterus, Dr. Dyer's case of abdominal section in	52	330
——— of the uterus, Mr. Haviland's case of	47	276
Ruptured perineum, Mr. I. B. Brown on	49	322

Saccharine urine, Dr. Venables on	48	76
Sarracenia purpurea, Dr. Grant on its efficacy in small-pox	49	30
——— purpurea, Dr. Miles' cases of small-pox treated by	47	12
——— purpurea, Dr. Morris on	47	7
——— purpurea, Dr. Taylor on its efficacy in small-pox	49	31
——— purpurea, Dr. Watson on	49	33
Scabies, Dr. Decaisne's new and speedy method of treating	51	275
——— Dr. Lowe on the treatment of	51	275
——— Dr. Nicholls on the treatment of	51	274
——— M. Gudden's recent researches on	52	251
——— M. Hebra's opinions on	52	255
——— Mr. Kendall on the treatment of	51	276
——— Mr. Strutt on the treatment of	51	276
Scarlatina, Dr. Addison on	52	20
———— Dr. Cummins's remarks on the use of sulphites in	51	34
———— Dr. Fenwick on condition of stomach and intestines in	50	9
———— Dr. Hamilton on the treatment of the throat affection in	48	317
———— Dr. Murchison on the etiology of	50	1
———— Mr. Blyth on the chlorine treatment of	49	7
———— Mr. Milton on the treatment of	52	27
———— complicated with rheumatic and cardiac affections	47	1
———— formula of the chorine mixture for	47	xiv
———— influence of age on the liability to	50	8
Sciatica, Dr. Fuller on the treatment of	49	387
Sclerotitis, Dr. Lawrence on its treatment by morphia	47	245
Scrivener's palsy, Mr. Solly on	51	41
"Sealing," Mr Holt's method of treating ulcers of the extremities by	47	220
Sea-sickness, Dr. Alston on	48	369
— tangle tents, Prof. Simpson on	50	354
Secale cornutum, Dr. Beck on the administration of	48	244
Secondary contagion, Mr. Hill's cases of syphilis communicated by	50	281
——— contagion, Mr. Solomon's case of infantile syphilis from	50	290
Section of the ciliary muscle versus iridectomy	50	252
Separation of the epiphyses of long bones, Mr. Hutchinson on	50	131
Sesquichloride of iron, its efficacy in consumption	50	54
———— of iron, its efficacy in ulcers of the nails	48	143
Shoulder, dislocation of, Mr. Salmon on reduction of	49	98
——— presentations, Dr. Christie on	50	297
Sickness of pregnancy, Dr. Cassells on the	51	334
Silver, nitrate of, Mr. Ellis on the action of	48	312
—— nitrate of, Mr. Higginbottom on the new preparations of	48	354
Silver-wire ligatures, Mr. Davies on	47	164
Skin, Mr. Wilson on the parasitic affections of the	49	168
—— diseases from gout or alcoholism, Dr. Ross on the treatment of	48	139
—— diseases, syphilitic, Dr. Squire on the diagnosis of	51	322

VOL. PAGE.
Sleep, Dr. Behrend on the action of bromide of potassium in inducing 50 xxxv
Sleeplessness, Dr. Hammond on the treatment of 52 74
Small-pox, Dr. Bowen on the prevention of pitting after 48 164
——— Dr. Grant on sarracenia purpurea in 49 30
——— Dr. Miles' cases treated by sarracenia purpurea 47 12
——— Dr. Morris on sarracenia purpurea in 47 7
——— Dr. Taylor on sarracenia purpurea in 49 31
——— Dr. Watson on sarracenia purpurea in 49 33
——— Mr. Higginbottom on the prevention of pitting after 48 165
——— on the prevention of pitting in 47 222
Smoking, Dr. Banks's case of paralysis of the hand and forearm from 50 44
——— M. Decaisne on its effect on the heart 50 50
——— as a predisposing cause of fatty heart, Dr. Kennedy on 50 47
Snake-bite, Dr. Berncastle's treatment of 48 350
——— Dr. Shaw's case of 48 351
Solution of calculi in the kidney and bladder, Dr. Roberts on 51 249
Sore nipples, Dr. Castle's treatment of 48 272
——— Dr. Mc Cormac's treatment of 51 361
——— Dr. Eastlake's treatment of 51 362
——— Mr. Storer's treatment of 51 362
Sores, Dr. Schreber on the use of clay as a dressing for 48 159
Sound, uterine, with moveable head, Dr. Earle's 50 321
Speculum for the eye, Mr. Lawrence on an improved 52 276
Sphacelus, anæmic, Dr. Myrtle's case of 48 159
Spinal affections, Mr. Hunter on the hypodermic treatment of 52 87
Spine, Dr. Dick's cases of pain of the 52 61
Spirituous liqours, Dr. Jones on the acid and sugar in 49 392
Spirometer, Dr. Bowman's method of constructing a cheap 48 47
Staphyloma, Mr. Critchett on the operation of abscission in 48 166
Staplyloraphy, Mr. Collis on 51 213
Steel, Mr. Milton on its use in eczema 52 230
Sterility, Dr. Gream on dilatation versus division of the cervix uteri for 51 391
Stillicidium, Mr. Bowman's treatment of 49 191
Stimulants in fever, Dr. Gairdner on the use of 51 7
——— in fever, Dr. Wilks on the use of 51 1
——— in fever, Mr. Leach on the use of 51 23
——— in fever, Editor of Lancet on 51 4
——— in serious cases of acute disease, Dr. Beale on 48 17
Stomach and intestines, condition of in scarlatina 50 9
——— pump, Dr. Hatrick on the use of the 48 346
Stone in the female, Dr. Humphry on 50 172
Strabismus, Dr. Soelberg Wells on 47 238
——— Dr. Todd's method of operating for 48 179
——— M. Graefe's operation for 50 216
——— Mr. Critchett's operation for 50 216
——— Mr. Hart's case cured without operation by the use of prisms 50 217
——— Mr. Hulke on the operations for 50 215
——— Mr. Lawson on its treatment at the Royal Ophthalmic Hospital 48 182
——— extreme divergent, Mr. Solomon on the radical cure of 51 301
——— the Moorfields operation for 50 215
Strangulated femoral hernia, Mr. Hewett on 47 182
——— hernia, its reduction after failure of taxis 47 184
Stricture, Mr. Smith on a case of death following the use of Holt's dilator 51 251
——— catheter, Dr. Watson's new probe-pointed 48 122
——— of the lachrymal duct, Mr. Walton on 48 184
——— of the rectum, cancerous, Mr. Curling on 51 222
——— of the urethra, Dr. Rutherford on Holt's treatment of 52 215
——— of the urethra, Dr. Smyly's treatment by the "more immediate plan" 47 199
——— of the urethra, Mr. Holt's method of forcible dilatation of 47 203
——— of the urethra, Mr. Holt's cases treated by splitting 50 178
——— of the urethra, Mr. Thompson on gradual distension at a single sitting 47 201
Strictures, Dr. Cruise on the use of the endoscope in 51 259
Strumous ophthalmia, Dr. Price on the use of tartarised antimony in 50 228
Strychnia, Prof. Bellini on the antidotes for 48 390

	VOL.	PAGE.
Strychnia, Dr. Fleming on the internal exhibition of	47	375
Strychnine, M. Kurzak on tannin as an antidote to	47	376
Subcutaneous injections, Prof. Nassbaum on the danger of	52	91
Substernal tenderness as a sign of acquired syphilitic taint	49	251
Suction-curette, Mr. Teale's, for the extraction of soft cataract	50	241
Sugar, Dr Pavy on its assimilation by the liver	52	113
——— diabetic, tincture of iodine as a test for	48	80
——— in spirituous liquors	49	392
Sulphites, Dr. Cummins on the treatment of scarlatina by	51	34
——— Dr. de Ricci on their efficacy in catalytic diseases	50	23
——— alkaline, Dr. De 'Ricci on the treatment of zymotic diseases by	52	1
Sulphuric ether and ammonia efficacy of as an antispasmodic in typhoid fever	50	30
Suspended animation, Dr Richardson on the treatment of	47	359
Sutures, Mr Clover's "Telegraphic," for closing wounds	47	163
——— Mr. Smith on horsehair as a substitute for wire	47	167
Sweating feet, M. Martin on prevention of the smell of	52	264
Sycosis, Dr. Masse on the action of creasote in	51	273
——— menti, vel mentagra, Dr. Laycock's treatment of	51	271
Syndectomy, Dr. Bader on the operation of	48	177
——— and inoculation, Mr. Lawson on	50	229
Syphilisation, Dr. D. J. Simpson on	51	312
——— Prof. Boeck's method of practising	52	307
——— Mr. Gascoyen on	51	309
Syphilis, Dr. Wilks on its treatment	47	235
——— Prof. Boeck on its treatment by syphilization	52	304
——— Editor of Med. Times and Gazette on the therapeutics of	51	303
——— Mr. Dunn's cases treated without mercury	51	326
——— Mr. Fleischmann on the modern treatment of	48	222
——— Mr. Hill on prolonged incubation period of, and secondary contagion	50	281
——— Mr. Langston Parker on its treatment by the mercurial vapour-bath	50	289
——— Mr. Pollard on its treatment by mercurial vapour	50	234
——— and its treatment, Dr. Veale on	50	277
——— constitutional, Dr. Brodrick on substernal tenderness as a sign of	49	251
——— infantile, Mr. Vose Solomon's case of, from secondary contagion	50	290
——— inherited, Mr. Hutchinson on the signs of	48	224
——— secondary, Mr. Allingham on the communicability of	51	316
——— secondary, Mr H. Lee on the modes of communication of	51	317
Syphilitic affections of internal organs, Dr. Wilks on	48	217
——— and non-syphilitic skin-diseases, Dr. Squire on the diagnosis of	51	322
——— eruptions, Dr. Tilbury Fox on	50	291
——— eruptions, M de Meric on	47	297
——— lichen and chancre attended with intense pruritus	51	325
——— sore throat, Dr. Thorowgood on chlorate of potash in	47	226
——— tubercle of the eyelid, Mr. Vose Solomon on	48	343
——— virus, Mr. Gascoyen on the unity of the	51	308
Syphon-trocar and hooked canula for ovariotomy, Mr. Wells's	49	302

Tape-worm, Dr. Brunton's treatment by kamela and male-fern combined	51	106
——— Dr. Constable's case treated successfully by oil of male-fern	52	119
Tannin, M. Woillez on its use in diseases of the respiratory organs	48	52
——— in inflammatory affections of the conjunctiva	48	186
——— as an antidote to strychnine	47	376
Tapeworm, Dr Fleming on the mode of administering the male-fern oil	47	115
——— Dr. Morris' case treated by the areca nut	47	116
——— Dr. Ogle on the oil of male-fern in	47	111
——— Dr. Rendle on the use of oil of male-fern in	49	xx
Tapping of the cornea in glaucoma	49	217
Tar, Dr. M'Call Anderson on its use in psoriasis	52	241
Tartar emetic, its use in the treatment of nævus	47	178
——— emetic plaister, M. Mailhe's formula for	48	359

	VOL	PAGE.
Tartarised antimony as a remedy in strumous ophthalmia ..	50	228
Tartrate of antimony as an oxytoxic	47	265
Telegraph sutures for closing wounds, Mr. Clover's	47	163
Tendinous rheumatism, Dr. Renard on the use of sulphur in ..	49	19
Tents, Prof. Simpson on the dried stems of the sea-tangle for .	50	354
Tetanus, Dr. Fleming on the treatment of	47	51
——— Mr. Adams's case treated by ice to the spine	50	35
——— Mr. Clarke on the pathology of	50	31
——— Mr. Tyrrell on the local application of tobacco in	50	32
——— from burn, Mr. Tyrrell's case treated by the application of tobacco	49	58
——— puerperal, Dr. Philipson on	52	347
——— puerperal, Mr. Blackshaw's case of	52	350
——— traumatic, Dr. Barton's case treated by nicotine	48	34
——— traumatic, Dr. Lassing on the use of electricity in	47	54
——— traumatic, Mr. Holmes and Dr. Ogle's case treated by nicotine	49	53
——— traumatic, Mr. Tufnell on its treatment by nicotine ..	47	46
Thigh, amputation of, by Teale's operation, Mr. Porter on.. ..	49	99
Thrombosis, Mr. Cooper on	47	297
——— and embolia of lying-in women, Dr. Barnes on	48	273
——— spontaneous, Mr. Tuckwell's case	49	135
Thumb, Mr. Wordsworth on dislocation at the metacarpo-phalangeal joint	48	96
Tincture of iodine, its use in the treatment of urethritis	48	132
——— of gelseminum, (American), its efficacy in neuralgic affections	49	68
——— of larch, its use in chronic bronchial affections	49	75
Tinea, Bazin's treatment of	49	170
——— Mr. Milton on the treatment of	52	223
Tobacco, M. Decaisne on its effect on the heart	50	50
——— Mr. Hutchinson on cerebral amaurosis connected with the use of	50	274
——— Mr. Tyrrel on its application in a case of tetanus from burn	49	58
——— its local application in the treatment of tetanus	50	32
——— smoking, Mr. Wordsworth on amaurosis produced by ..	48	212
——— smoking as a cause of optic atrophy	48	213
Toe-nail, ingrowing, efficacy of sesquichloride of iron in	48	143
Tongue, Prof. Syme on excision of the	51	194
——— tumour of, treated by the galium aperinum	50	141
——— ulcer of, Dr. Leared's treatment of a case of	52	261
Tonsils, enlarged, Dr. Mackenzie on the treatment of	50	143
——— enlarged, Dr. Smyly's treatment of	51	220
Toothache, Dr. Watson on carbolic acid in	51	xvii
Tophaceous deposits connected with gout	47	16
Tracheotomy. Mr. Porter on importance of excising a small portion of windpipe	49	140
Transfusion, Dr. Hewitt on the operation of	51	347
Trephining for compression of the brain, Mr. Switzer on	52	158
——— in injuries of the head, Dr. Ashhurst on	50	129
Trichiasis, Dr. Wright on the operations for	51	278
——— Mr. Critchett on the treatment of	47	251
——— tonsurans, Bazin on the treatment of	49	172
Trichina spiralis, Dr. Müller on	49	27
Trichiniasis, Dr. Mosler on the efficacy of benzine in	51	277
Triplets. Mr. Furnivall's case of	48	257
Truss, Mr. Salt on a new form of	49	154
——— pads for hernia. Mr. Wood's new	49	147
Tubercle, Dr. Bennett on the pathology of	48	53
——— syphilitic, of the eyelid, Mr. Solomon on..	48	333
Tuberculosis, Dr. Bennett on the treatment of	48	55
——— Dr. Jones on inflammation as an exciting cause of ..	51	58
Tumours, Mr. Bryant on the pathology and diagnosis of	48	20
——— cystic, of the vagina, Dr. Barnes' case of	47	261
——— fibrous, of the uterus, Dr. Duncan on the source of hemorrhage in	49	320
——— fibrous, of the uterus, Mr. Brown on the treatment of ..	49	316
——— fibrous, of the uterus, Mr. Brown's operation for . ..	51	362
——— malignant fibro-cellular, Dr. Buchanan on	49	186
——— mammary, Mr. Bryant on the diagnosis of	50	374
——— of the jaws, Prof. Fergusson on the operations for ..	52	147
——— of the tongue, Mr. Bulley on galium aperinum in	50	141

	VOL.	PAGE.
Tumours, ovarian, Dr. Clay's cases of extirpation of	50	356
—— recurring fibroid, Mr. Bryant on	48	23
—— uterine, Dr. Gairdner's cases simulating ovarian disease	50	342
—— uterine fibrous, Dr. Routh on the diagnosis of	50	330
—— uterine fibrous, Dr. Routh on the treatment of	50	336
—— vascular, of the meatus urinarius	49	358
Turning in cases of disproportion, Dr. Inglis on	51	337
—— in cases of shoulder presentations, Dr. Christie on	50	297
—— in certain cases of labour, Dr. Hewitt on the advantages of	50	294
—— versus forceps, Dr. Graily Hewitt on	50	294
Tympanum, Mr. Hinton on a simple method of inflating the	49	250
Typhoid fever, Dr. Henderson on the use of hydrochloric acid in	48	68
—— fever, Dr. Osborn on sulphuric ether and ammonia as an antispasmodic	50	92
—— fever in Penicuik, Dr. Thin on	51	13
Typhus and typhoid fevers, Dr. Gairdner on the treatment of	51	7
—— and typhoid fevers, Dr. Wilks on the use of stimulants in	51	1
—— fever, Dr. Christison on	49	1
—— fever, and its prevention, Dr. Mapother on	50	14

Ulcer of the tongue of twenty years standing, Dr. Leared's cure of	52	261
Ulceration of the cornea, Mr. Carter on iridectomy in	48	192
Ulcers, Mr. Turner on the use of carbolic acid in	48	368
—— chronic, Mr. Skey on the treatment of	49	189
—— of the legs, Mr. Holt on their treatment by "sealing"	47	220
Uræmia, Dr. Harley on	49	397
Uranoplasty, Mr. Collis on	51	213
Urates, Dr. Harley on the pathology of deposits of	50	80
Urea, Dr. Harley on	49	86
—— amount daily discharged in the healthy urine of man	47	364
Urethra, stricture of, Mr. Holt's treatment of	50	178
—— stricture of, Mr. Rutherford on the treatment of	52	215
Urethritis, Dr. Cruise on the treatment of	51	258
—— M. Oscar Max on the use tincture of iodine in	48	132
Uric acid calculus, Dr. Harley on the treatment of	50	83
Urinary analysis in obscure disease, importance of	50	92
—— affections, Dr. Hassall on the action of perchloride of iron in	51	150
Urine, Dr. Clark on detecting small quantities of albumen in the	50	169
—— Dr. Harley on the colouring matters of the	50	89
—— Dr. Harley on the	49	85
—— Dr. Pavy on the influence of assimilation on the	48	323
—— Dr. Pavy on the influence of assimilation on the	47	124
—— M. Bouchardat on the methods of its examination in disease	47	123
—— Mr. Donovan's simple method of estimating the acidity or alkalinity of	49	82
—— amount of urea daily excreted in health	47	364
—— incontinence of in boys, Mr. Adams's treatment of	50	389
—— retention of, on certain errors in the diagnosis and treatment of	47	192
Urohæmatin, excess of in the urine in various diseases	50	92
Uterine action in labour, Dr. Brown on the use of ergot in inducing	52	324
—— discharges, fetid, Dr. Matthews Duncan on	47	336
—— fibrous tumour, Dr. Duncan on source of hemorrhage in	49	320
—— fibrous tumours, Dr. Routh on the diagnosis of	50	330
—— fibrous tumours, Dr. Routh on the treatment of	50	336
—— fibrous tumour, Dr. Sadler's case of removal of	49	319
—— fibrous tumour, Mr. I. B. Brown on the treatment of	49	316
—— fibrous tumours, Mr. Brown's operation for	51	362
—— hemorrhage, Mr. Baker Brown on incision of the os and cervix for	50	315
—— hemorrhage after delivery, Mr. Macdonald on	47	278
—— hemorrhage after delivery, Dr. Lumley Earle on	50	310
—— hemorrhage from fibrous tumours and polypi, Mr. Baker Brown on	50	314
—— pessaries, Dr. Tanner on medicated	48	310

	VOL.	PAGE.
Uterine pessary, Dr. Wilkie's double-jointed	48	307
—— polypi, Prof. Simon's mode of extirpating large	48	291
—— polypi, Dr. Marion Sims on the removal of	51	381
—— porte-caustique, Dr. Lente's new	49	346
—— sound with moveable head, Dr. Earle's	50	321
—— tumour simulating ovarian disease, Dr. Gairdner on ..	50	342
Uterus, Dr. Duncan on state of the internal surface after delivery ..	48	287
—— Dr. Emmet on a radical operation for prolapsus of ..	52	374
—— Dr. Dyer's case of rupture of the	52	330
—— Dr. Gill on the action of ergot of rye on the	47	264
—— Dr. Hewitt on the action of ergot of rye on the	47	262
—— Dr. Kelly on inversion of the	49	264
—— Dr. Lumley Earle on the introduction of the hand into the cavity of	50	312
—— Mr. Haviland's case of rupture of	47	276
—— Mr. Sheppard on inversion of the	49	268
—— bladder, and rectum, Mr. Brown on prolapsus of the ..	49	322
—— irritable, Dr. Griffith on the treatment of	49	330
—— physiological action of in parturition	49	338
—— prolapsus of, Mr. Davies on the treatment of	49	327
—— retroflexion of, Dr. Graily Hewitt on the treatment of ..	48	260
—— retroflexion of, Dr. Routh on the treatment of	48	261
—— retroflexion of, Mr. Baker Brown on the treatment of ..	50	351
—— rupture of, Dr. Crighton on gastrotomy in	50	304

Vagina, Dr. Bennett's new method of plugging	48	258
—— atresia of, Mr. Brown's case of retained menses from ..	48	295
—— case of cystic tumour of the	47	261
—— ruptured, Dr. Bell's case of	48	225
Vaginal injections, Dr. Rasch's instrument for	51	394
—— lithotomy, Dr. Aveling and Mr. Spencer Wells on ..	47	215
—— lithotomy, Mr. Lane on	47	211
Valvular lesions of the heart, Dr. Flint's general account of ..	47	69
Varicose veins, Mr. Skey on the treatment of	49	126
Varicocele, Dr. Packard on the radical cure of	48	131
—— Mr. Holt on its treatment by the wire ligature	48	130
Vascular tumour of the meatus urinarius, Dr. Griffith on cases of ..	49	358
Venice turpentine, solution of, as a dressing for wounds	52	262
Veratrum viride, Dr. Edwards' case of poisoning by	47	372
Verbascum thapsus, or mullein, its use in chronic bronchitis ..	47	xviii
Vesico-uterine fistula, Mr. Lane on	47	283
—— uterine fistula, Mr. Lane's case of	49	310
—— uterine fistula. Mr. Pollock on the operation for	49	315
—— utero-vaginal fistula, Mr. Spencer Wells on	47	291
—— vaginal fistula, Dr. Banon on the surgical treatment of ..	51	395
—— vaginal fistula, Dr. Oldham on	47	281
—— vaginal fistula, Dr. Tyler Smith on	47	281
—— vaginal fistula, Mr. I. Baker Brown on	47	279
—— vaginal fistula, Mr. Bryant on the treatment of	52	371
Vinum ferri, its use in consumption	50	53
Vulcanite, red, in dentistry, Dr. Wells on the ill effects of	48	357

Wasting palsy, Dr. Radcliffe on	51	44
Weight and height in the human body, relations of	48	373
Willow-bark, M. Cazin on the febrifuge properties of	50	31
Wire compress, a substitute for the ligature, Mr. Dix on the ..	47	158
—— compress as a substitute for the ligature, Mr. Dix on the ..	50	113
—— ligature, Mr. Holt's cases in which it was left in the wound ..	50	125

	VOL.	PAGE.
Wire ligature, its use in the treatment of varicocele	48	131
—— ligature, operation for the cure of varicocele by	48	130
—— loops as substitutes for pessaries, &c., Dr. Clay on ..	49	253
Wood-oil, or Gurgina balsam, in gonorrhœa	51	329
Worms, round and thread, Dr. Anderson on the use of santonine in	49	xx
Wounds, M. Kerner on solution of Venice turpentine as a dressing for	52	262
——— Mr. Holmes on the exposure of articular cartilages in ..	51	176
——— into large joints, Dr. Monro on	50	110
——— penetrating, of the knee-joint, Dr. Newman on irrigation in	51	177
Zymotic diseases, Dr. de Ricci on sulphites and hyposulphites in ..	49	9
——— diseases, Dr. de Ricci on the use of the sulphites in ..	50	23
——— diseases, Dr. Mapother on the prevention of	50	13
——— diseases treated by alkaline sulphites, Dr. De 'Ricci on ..	52	1

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